THE INHERITANCE OF ACQUIRED CHARACTERISTICS

The Inheritance of Acquired Characteristics

BY

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TO

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THE HIGHLY MERITORIOUS DISCIPLE OF THE DOCTRINE THAT ACQUIRED CHARACTERISTICS ARE HEREDITARY, AND AN INDEFATIGABLE CHAMPION OF THE TRUTH, WITH AN EXPRESSION OF THE SINCEREST ADMIRATION OF

THE AUTHOR

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PREFACE

In the most widely read of the present-day textbooks dealing with the Science of Inheritance—a science which just now, so to speak, has become of age—Bateson, Baur, Haecker, Hart, Johannsen, Macfie, Strasser, Teichmann, Thomson, De Vries and Ziegler more or less strongly oppose the theory of the Inheritance of Acquired Characteristics. Even those writers who, only a few years ago, admitted that there are certain facts in favor of the inheritance of acquired characteristics (R. Goldschmidt in the *first* edition of his *Einführung in die Vererbungswissenschaft,* 1911—"Introduction into the Science of Inheritance") in later editions of their works expressed themselves much more guardedly. Fritz Lenz, reviewing the third edition of Goldschmidt's book, apparently approves of the author's disregard of "Kammerer's Celebrated Salamander Experiments," explaining that this omission is equally as justified as that of Guthrie's experiments with interchanging the ovaries of black and white hens.

Other writers, decided followers of the theory of the inheritance of acquired characteristics, did not treat this subject—as if they were forced to do so, to be taken seriously—within the scope of their textbooks, but rather chose to deal with the problem in special treatises (for example, L. Plate who preferred to treat this subject in a separate volume on *Selektions-*

prinzip und Probleme der Artbildung—"The Principle of Selection and the Problem of the Development of the Species"—rather than within the scope of his *Vererbungslehre*—"The Science of Inheritance"). Another author who was always regarded as an adherent, in his latest work so fervently expounds an affirmative point of view regarding the opposing theories of Selectionism, Mendelism, and Mutationism that one asks oneself: Now, is he fighting for or against the theory of the Inheritance of Acquired Characteristics? (O. Hertwig, *Das Werden der Organismen*—"The Development of the Organisms").

Even more outspoken than in writing, if possible, the inheritance of acquired characteristics has been opposed at the congresses of the *Deutsche Gesellschaft für Vererbungswissenschaft*—"German Society for the Science of Inheritance"—Vienna, 1922; Munich, 1923. The theory was also opposed at the respective congresses of the "Geneticians." In Vienna, 1922, for example, it was even formally resolved that it could be considered a certainty that there is "no such thing as the inheritance of acquired characteristics." This naturally led to an extraordinary impotency in raising and solving problems, and became exceptionally marked by an utter collapse amounting almost to an admission of complete bankruptcy, as far as the possibility of explaining the phenomenon of human inheritance was concerned.

Squarely facing the question as to how such a one-sidedness, such a rigid dogmatism, could prevail, the answer can be found partly in the development of our science and partly, even, in the development of our political situation.

Within the realm of science, it was the coincidental coming to the fore—or if referring to the publication of the writings, the close succession—of Weismann and Mendel, by which a discrediting of the inheritance of acquired characteristics was brought about. In this respect, perhaps an especially strong impression was created by the fact that the results of Weismann's and Mendel's theories contrasted with each other, as far as the effectiveness of the principle of selection was concerned, but both denied the inheritance of acquired characteristics. It cannot be brought out too strongly that it was not Mendel himself who came to this conclusion: he only described his crossing experiments and almost nothing else. It was his successors who came to far-reaching conclusions. Not to the same degree, yet intrinsically alike, the same holds true with Weismann who was "out-Weismanned" by his disciples.

Weismann, Mendel, and the scientific camps they founded amounted in the beginning to a very necessary and beneficial reaction after an epoch of an utter lack of criticism, which one had become resigned to as a matter to be taken for granted. It was in this period that anecdotes made the rounds: like the one of the bull whose tail had been cut off by a slammed stable door, and who, from that time on, could never be the sire of anything but tailless offspring; or that other story about the cow which knocked off one of her horns and thereafter bore calves only with limpidly hanging down horns. Gradually this reaction developed into a "reaction within itself"; that is, to a halt of progress, and to outspoken retrogression. Here one of those oscillations of fashion manifested itself to which the progress of scientific research work is just as little im-

mune as any other branch of human activity. For this reason today the theory of non-inheritance of acquired characteristics is taken for granted. In practically the same manner the inheritance of acquired characteristics formerly had met with general acceptance.

The non-inheritance of acquired characteristics doubtless suits our slow process of thought better than the opposite opinion voiced here. Einstein recognized the main reason of the opposition in our habit of thinking when I, as a guest at his table, talked this matter over with him. The assumption of the inheritance of acquired characteristics necessitates a much more difficult process of relative thinking. It demands that we look at the living being, and the cycle of its life from the conception to ultimate development and back, in relation to the exterior world. We, of course, are much more apt to think absolutely, because it is so much more convenient, and this, in turn, leads us on to envisage the cycle of life without any relation to the rest of the world, which makes it so much easier to consider the course of inheritance as something utterly unchangeable, independent from foreign influences.

But the present-day negativistic fashion in still another respect must be branded as "reaction." The Great War—after an only too short-lived swing in the direction of brotherhood of mankind and lasting world peace—almost all over the world, but especially in Central Europe, has resulted in an intensification of nationalistic and racial consciousness. This nationalistic self-confidence, however, is opposed to the theory of the inheritance of acquired characteristics, because it teaches that inheritance—the passing on of proudly

referred-to race and caste characteristics inherited from forebears—is not everything, but that one's economical situation and environment (people among whom one moves, and especially among whom one grows up) have a very great influence on the individual. This tremendous influence reaches even as far as the characteristics of one's origin, one's descent. Racial boundaries are omitted. National contrasts seem likely to be abolished by environment and education not only for the duration of the life of the individual, but also with generative after-effects. A time when national and racial merits are so distinctly brought out; a period, or during an episode, when the civilization of one's own race is demanded even at the expense of another race's civilization—instead of the two races coöperating with each other—such a time is not at all propitious for the propagation of such a theory as the one of the inheritance of acquired characteristics.

But with the inheritance of acquired characteristics, the proud edifice of humanity's progress stands and falls, at least in so far as this progress is not only accomplished by extrinsic traditions, but intrinsically and organically achieved; that is, genuine progress, "upward development" (*Höherentwicklung*—Goldscheid). All progressive measures, at home and in school, private and public welfare endeavors, education, administration and government, are endowed with a new and more far-reaching importance when dealing with the theory of the inheritance of acquired characteristics. Only then all these institutions serve not only the fleeting moment and the individual, but also eternity and the generation. No wonder, there-

fore, that everybody professing reactionary tendencies, in private and public life, fervently combats the contention that personally acquired characteristics somehow, and at some time, can be transmitted.

"The Republics of Sciences" (as European universities glowingly love to refer to themselves, in spite of mostly being reactionary and autocratic in the extreme), consciously, semiconsciously, and unconsciously have frequently joined the opposition against such a revolutionary doctrine. The more or less clear impression that, in the fight against the inheritance of acquired characteristics, there is more at stake than a mere decision in the realm of the theory of inheritance, and that the object of the fight completely embraces the theory of evolution (the theory of transmutation, and the development of higher species from lower which could not be attained without an inheritance of acquired characteristics), this justifiable impression is the reason why the controversy on the part of scientific, reactionary combatants has grown into an extremely fierce and relentless fight.

All this tends to prove that a "reaction against reaction" is a crying need. This need is just as insistent in the days after Weismann, when an unjust and prejudiced opposition towards the theory of the inheritance of acquired characteristics must be fought, as in the days before Weismann, when the non-critical sweeping acceptance of everything pertaining to the inheritance of acquired characteristics was gullibly swallowed. Provided that the appearance of things is not deceiving, the turning of the tide has already set in. More and more voices are heard insisting that the unparalleled ascension of Mendel's research work has

reached the zenith, that Mendelism, and the methods of the "theory of exact inheritance" (W. Johannsen) as well as the theory of mutation (De Vries and his disciples) are facing failure wherever it is necessary to explain never-to-be-discussed-away manifestations of the descent of races, species, and groups.

There is a multiplication of experiments which essentially deal with the inheritance of acquired characteristics; but up to now there seems to be a deplorable lack of courage to call the thing by its true name as soon as affirmative reports would have to be submitted. These affirmative results are usually camouflaged with more or less high-sounding, but at the same time, more than less misleading appellations; such as, "cumulative after-effect" (Alverdes, 1921 a *), "oscillating mutations" (Cuénot), "transgressive œcologisms" (Lang, 1909), "enduring modifications" (Jollos), and what not. But one nice day all these disguises will fall and, chastened, we will return to the ideas of Lamarck, Goethe, and Darwin.

To prepare for this turning of the tide, to do all that is possible once the time is ripe, to offer an "easy balance" (this expression will surely be interpreted differently) to the negativistic point of view which usually manifests itself in the present-day literature on inheritance, as pointed out before, seems to me quite an enticing and, as I believe, quite a worthy task. I assume this task the more readily since I will have to

* See "References."—As it is not always possible by just mentioning an author's name to know which of his writings are quoted, especially in such cases where the author in question has written more than one publication, the year of the publication is mentioned. Should there be more than one of his papers published in the course of one year's time, a letter follows the numerals.

refer to my own research work which my opponents fight with all dialectic feints imaginable, and because I was in doubt of the inheritance of acquired characteristics before my own research work had grown to such proportions as to irrevocably convince me of the truth of the theory I am now advocating.

I took care not to disregard the results of other scientists, and I trust that I have succeeded in enlightening my readers on the point of view which my opponents take. I sincerely hope that, after reading this book, it will be freely admitted that I, as an *advocatus diaboli,* have given a painstaking presentation of the viewpoint of my adversaries. I do not pretend that my comparatively limited treatise is complete, either in regard to my own research work or to that of others, or in regard to positive or negative facts and interpretations.

Certainly I expect this inadequacy on my part to be counted against me. However, in consideration of the present-day impatient and overfed ranks of readers —scientific and non-scientific—who do not favor the reading and studying of portentous volumes, I came to the conclusion that to add a more extensive treatise (even if written from an entirely different point of view) to those already existing would not assure for my book the large audience which, in the interest of the goal I aim at, seems desirable.

On the other hand, I hope that I have not omitted anything of principal and basical importance. As far as there may prevail incompleteness at all, this only has reference to the possibility that, in almost every important chain of experiments, each essential manifestation of inheritance could have been substantiated

with even more instances. In this respect, my book may be remiss in an overwhelming weight of proof; but, on the other hand, this succinct treatise is not overburdened with tiring details. But the goal will be achieved in spite of all this if I have succeeded in my endeavor to throw light on the problem of the inheritance of acquired characteristics, with all its vital and principal phases.

There is another peculiarity in my book which most probably will challenge disapproval, and for this reason necessitates an anticipatory justification. I appeal with this volume—just as I have done for quite some time—to the laity also. For this reason I have tried my best to replace scientific and Latin names with more colloquial expressions. Drs. G. Kingsley Noble and Robert Cushman Murphy of the American Museum of Natural History, New York City, and Prof. W. J. Goldfarb of the College of the City of New York, were kind enough to lend me their valuable assistance in this and other respects. My treatise, however, is surely not popular in such a way that it could be considered "light reading matter," although it affords anybody with education and everybody endowed with sufficient reasoning power the opportunity to consider the subject, and to draw his own independent conclusions.

Just this is the point: I am sure to be told where, by necessity, I will be unsuccessful, where I will fail to realize my expectations, because only my colleagues would be able to arrive at a well substantiated judgment which would be in conformity with the present stand of science. As a matter of fact, especially as far as the subject in question is concerned, very far-reaching

and highly detailed knowledge is necessary if criticism and anti-criticism are not to miss their mark by too wide a margin.

On the other hand, all such far-reaching and highly detailed knowledge has not always served to save the problem of the inheritance of acquired characteristics from the weightiest, almost insurmountable prejudices. But even admitting that the best informed, the most intelligent layman is unable to formulate an accurate opinion which could weather the storm of scientific scrutiny, it would not be the first time that the laity's interest in a question would make itself beneficially felt as an agent of fermentation, stimulating the sluggish metabolism of scientific reasoning. A certain pressure of the public's opinion, a certain contrast between the voice of the laity, and established academic-scientific conceptions—be they rightly established or not—has often enough beneficially enhanced the desire for truth.

I am full of joyful expectations again to observe a similar process of stimulating science by profoundly interesting the laity in the subject in question, analogous to those developments brought about by the writings of Carl Vogt, Buechner, Boelsche, Haeckel, and Ostwald. May such a development induce the best man in this realm of science to probe anew, and from the very bottom, the facts which are underlying the inheritance of acquired characteristics!

PAUL KAMMERER.

New York, May, 1924.

A. BIOLOGICAL PART

CHAPTER I

INHERITED AND ACQUIRED CHARACTERISTICS

WE, all, are possessors of a certain number of characteristics which mark us as species, as members of a certain race or a certain family, and as individuals. Most of our physical and mental characteristics we have inherited from our parents, from our grandparents, from our forebears in the dark past. We have acquired only a *few* of these characteristics in the course of our individual life.

We may claim only such characteristics as individual which we acquired during the course of our lifetime, and not those which fell to our lot simply because we are members of a certain species, race, or family. Even though the latter may manifest themselves in us to a degree, and in a combination quite extraordinary, just the same all these characteristics are only inherited and not of our own making.

In his celebrated verses:

> In me, my father's build prevails,
> His sternness I inherit,
> With mother's love for spinning tales,
> And with her happy spirit.
> —*Translated by George S. Viereck.*

Goethe credits his parents for his splendid gifts; in a scientific sense, renouncing with it his own personality. Only when one comes to appreciate that which

he added to the gifts with which he was endowed at birth—that he succeeded in intensifying inherited determinants (tendencies) to the highest degree of perfection—one has done justice to Goethe's true self.

Suppose I have to thank my father for a high, well-molded brow and an indomitable will, and my mother for an aquiline nose and musical talent. From my grandfather, on my father's side, I inherited big, bright brown eyes and auburn hair; from my grandmother, the tendency of becoming prematurely gray. To these and other family characteristics are added the characteristics of my race—the white skin, the orthognathous jaw, etc.—as well as the characteristics of a member of the species man; *i.e.,* the shape of my skull, my erect carriage, the faculty of an articulate language, and an intelligent employment of tools. All this I inherited, and did not acquire on my own merit.

But in case my face is tanned by the sun, or exercise of a certain kind hardens my muscles; a wound heals, leaving a permanently visible scar; my faculty of speech is enlarged by acquiring the use of foreign languages; or I improve my inborn musical talent by becoming proficient along this line, I am rightfully entitled to consider such characteristics as acquired by myself. Then I live up to the exhortation:

> What from your fathers' heritage is lent,
> Earn it anew, to really possess it.

But just this continuation where the forebear left off, the indispensability of organic traditions passed on to mold them into something new, the impossibility to create something out of nothing, makes it frequently difficult to distinguish between inherited and

acquired characteristics—to distinguish the old building from its new annex, so to speak.

To judge, for example, that our teeth—regarding their number, order and form of the individual teeth —were inherited is just possible, in spite of the fact that we are not born with a set of teeth, but only with a hidden tendency which fully develops later on. But we know that the little teeth lay already prepared in special cavities (*alveoli*) and, even though the infant is apparently born toothless, he does not have to learn to grow teeth nor to practice growing them. Out of their own power, the milk teeth break through the jaw. We are hardly in a position to aid this development or to alleviate the infant's pain by applying teething rings and other devices.

But even in regard to one's native tongue, it is more difficult to distinguish what has been inherited, and how much has been acquired. Would the infant learn to talk without being taught? Only those persons entirely uninformed would deny this question as a matter of fact, and would point to the observation that a child in a foreign country acquires knowledge of the foreign language apparently just as easily as the use of the language which his forebears and compatriots speak, and in which they were brought up. Now, is the ability to speak a language in its entirety an acquired characteristic? Or is there not an inherited disposition, an inborn tendency which could be compared to the little teeth, only that in the present case outside help is necessary to develop the inborn tendency?

It is hardly possible to arrive at a definite decision in this respect because, for experimental purposes only,

children cannot very well be brought up without being taught to speak. We are compelled, therefore, to resort to animal experimentation.

Lloyd Morgan observed that finches (*Fringilla cœlebs* and *Carduelis elegans*), and White Throats (*Silvia*) taken from the nest as helpless fledglings and brought up away from their own kind, nevertheless sang their native song, though it had lost some of its original volume and beauty.

Romanes (1888) reports that in destroyed Indian villages in South America and California, quite often little children were abandoned who, thanks to the abundant vegetation of the tropical clime, lived on fruits and roots. These children, it is claimed, out of their childish babbling, developed a new language. As this happened comparatively often, Romanes believes it to be an explanation of why so many different tribes of Indians speak entirely different and independent languages.

We may conclude, then, from this example, that every human being is a product of inherited and acquired characteristics. No individual could possibly exist without being forced by the circumstances of his life to independently enlarge his heritage. Other questions pregnant with meaning loom up from such a deduction; such as: is it possible that characteristics acquired by the individual sooner or later are passed on to the next generation? Is it possible that highly personal characteristics—under certain favorable circumstances, of course—are converted into family and racial characteristics?

That characteristics inherited from forebears can, in turn, be passed on to posterity has never been

doubted. With a certain approximate exactness, we even know today the law underlying this inheritance and the mechanism by which it is brought about. Inherited characteristics are subject to the so-called Mendelian Rules, discovered by Gregor Mendel, an Augustine prelate. However, within the scope of the present volume it is not intended to go into details in this respect, but merely draw upon them on fitting occasions.

The Mendelian Rules visibly express themselves in the kaleidoscopical harmony, the segregation and combination of a substance which is contained in the cell-nucleus, and which becomes clearly visible under the microscope after the addition of coloring matter. The "Chromatin" of the cell-nucleus is, therefore, considered the very substance of inheritance. The crystalline fragments ("Chromosomes") into which it is dispersed at every division of a cell are supposed to be the carriers, vessels, or at least the vehicles (R. Goldschmidt, Heider, and T. H. Morgan, 1919) of inheritable characteristics, respectively of the tendencies which, during the period of development, grow into mature characteristics.

There is neither space nor necessity for explaining all the infinitesimal details: any textbook on biology or any general outline of the doctrine of Inheritance, will furnish the details omitted here.

But what is the fate of personally acquired characteristics? Do they die with individuals or do they extend—at times at least—beyond the boundaries of the individual's life into the life of succeeding generations? Much depends on the correct answer to this question, as we will see towards the end of this book.

CHAPTER II

SLAVES OF THE PAST OR CAPTAINS OF FUTURE?

IF acquired characteristics cannot be passed on, as most of our contemporaneous naturalists contend, then no true organic progress is possible. Man lives and suffers in vain. Whatever he might have acquired in the course of his lifetime dies with him. His children and his children's children must ever and again start from the bottom.

Certainly, they may inherit material riches together with traditions, be they passed on verbally or in writing, but all this does not constitute real genuine wealth. No human brain is able to achieve anew all the accomplishments of all the previous generations. None of them would then be able to retain—even within the limited realm of a highly specialized sphere of knowledge—enough strength to continue building on the foundation of the laboriously achieved accomplishments of our forebears. Surely, if a definite scientific perception destroys our hopes of evolution in such a manner, we would have to bow before the truth, be it ever so disconsolating. A revolt against facts would be of no avail; meek submission is the only thing called for in such a case. But is the negative perception regarding the inheritance of acquired characteristics really definite? Is there never and under no circumstances an inheritance of the new?

If acquired characteristics are occasionally inherited, then it becomes evident that we are not exclusively slaves of the past—slaves helplessly endeavoring to free ourselves of our shackles—but also captains of our future, who in the course of time will be able to rid ourselves, to a certain extent, of our heavy burdens and to ascend into higher and ever higher strata of development. Education and civilization, hygiene and social endeavors are achievements which are not alone benefiting the single individual, for every action, every word, aye, even every thought may possibly leave an imprint on the generation. Of course, these traces will work in two directions. Shakespeare's words:

> The evil that men do lives after them;
> The good is oft interred with their bones,

in the light of present-day knowledge need rewriting, as traces left by our forebears may work in a degenerative as well as in a regenerative direction.

Wherever these passed on, acquired characteristics manifest themselves destructively, we will refer to them as "degenerative" inheritance, whereas we will speak of a "regenerative" inheritance if the characteristics passed on serve to enhance "upward development." It will make no difference whatsoever whether an inherited degenerative characteristic is ameliorated or a new regenerative characteristic is acquired, to consider such a development regenerative.

Now, is there, according to the way we live our lives, a degenerative inheritance and a regenerative inheritance? Is there such a thing as "progression" and "retrogression" by generation, according to our choice

and understanding? This question is of utmost social-political and race-hygienical importance; and if, with my introductory remarks, I have succeeded in arousing general human interest in this question, if I have succeeded in making it plain that we all are part of this question, whether we wish to be or not, then I shall feel that my readers are willing to follow me when I now draw the material for my answer to this question from a kingdom of creation which is inferior to ours.

CHAPTER III

THE IMPORTANCE OF BREEDING EXPERIMENTS

WE must needs descend below man, the species *homo sapiens,* because neither direct observation of family, nor statistical comparison of thousands of observations will yield the solution. The only way to bring a definite solution about is by methodical breeding experiments for which, of course, humans cannot be drawn upon. We are limited to the animal and vegetable kingdoms which, in a way, are to be preferred, because the members of these two kingdoms offer better possibilities of comparison than the intricate and confusing conditions and manifestations that humans present.

But what conclusions may we draw from inheritance in animals and vegetables? What may we deduce from the results of our research work within these two spheres below man, as far as regenerative inheritance is concerned? In that respect, we may readily depend upon the miraculous oneness of living nature. The law of nature, which manifests itself in the inferior kingdoms of the animal and vegetable, has proved to be valid for our superior species. For this reason, the greater part of our medical knowledge is substantiated by animal experimentation. And for the same reason, Gregor Mendel discovered his laws of heredity, experimenting with peas and wall-flowers in the garden

of the monastery in Bruenn, Moravia. Other naturalists corroborated his findings, experimenting with fowls, mice, guinea pigs, and other animals, eventually finding the same rules underlying the family trees of man (for example, the heavy underlip of the members of the House of Hapsburg; color-blindness, etc.). For this reason, I ask my readers to patiently follow my discourse, even though my investigations may strike one as being rather odd and dry, inasmuch as they are conducted on living beings apparently far removed from us; but, nevertheless, these investigations continually apply to man. If not in accordance with the special result, they apply in principle to man and are applicable to the life of man and the propagation of our species.

CHAPTER IV

EXPERIMENTS ON BUTTERFLIES

THE fundamental experiments concerning the inheritance of acquired characteristics were conducted on butterflies. If the chrysalis of the Nettle Moth (*Vanessa urticæ;* Fig. 1, center row), are kept in a refrigerator, the majority of butterflies emerging from it will be colored darker (*b, c*) than the normal specimens (*a*). This darkening shows in a general dimming of the prevalent color, and an enlargement of black spots which eventually merge into quite extensive areas. Both these observations are more noticeable on the male (*c*) than on the female (*b*).

It is a general experience that the male is more easily influenced by changes and much more sensitive to harmful influences than the female, the male being the progressive, and the female the conservative element in evolution.

A large part of the progeny of specimens with dimmed colors (*d*) are almost as dark as their parents (*b, c*); that is, darker than the grandparents (*a*) which were not influenced in any way. These offspring of specimens darkened by experimental methods even then show a dimmer color scheme of their wings when they themselves were not kept in a refrigerator, but left to development in normal room temperature.

The fundamental experiments of Standfuss (1898) developed in strict accordance with the observations described above. A similar experience occurred in experiments which E. Fischer (1901) conducted on the Great Tiger Moth (*Arctia caja;* Fig. 1, bottom row), who deviated from the experiment of his forerunner in that he did not expose the chrysales to frost for the whole period of their pupæ-stage, but only intermittently did he subject them to the influence of a much lower temperature than the experiment of Standfuss, about 17° F. The dimming of the prevalent colors (*b, c*) again manifested itself in an expanding and merging of dark spots (especially on the posterior wings) as well as in a gradual elimination of the net-like, white designs on the anterior wings. Both these changes were more apparent in the male (*c*) than in the female (*b*), an observation which repeated itself in the progeny raised in intermediate room temperature (*d*).

Another experiment analogous to the foregoing was the one conducted by Christian Schroeder (1903 a) on the Harlequin, also called the Gooseberry Moth (*Abraxas grossulariata;* Fig. 1, top row). Schroeder also succeeded in dimming the prevalent color and expanding the black, and in the elimination of yellow wing designs, not by frost but by heat. It is a frequently proved experience that extremes—in our case a very high and a very low temperature—bring about the same changes. Christian Schroeder exposed the Harlequin pupæ (chrysales) three times a day (each time for the length of one hour and a half) to a temperature of 102° F., and again observed that the changes brought about in such a way were

FIG. 1.—TOP ROW: THE GOOSEBERRY MOTH (*Abraxas grossulariata*)
(Chr. Schröder, 1903 a)

CENTRE ROW: THE NETTLE MOTH (*Vanessa urticae*)
(Standfuss)

BOTTOM ROW: THE GREAT TIGER MOTH (*Arctia caja*)
(E. Fischer)

a, Normal butterfly; *b,* female and *c,* male, with colors dimmed by change of temperature. *d,* Progeny of specimens with artificially dimmed colors but raised in intermediate temperature
(Przibram, 1910 a)

much more far-reaching as far as the male was concerned *(c)*, and were again to be observed in the progeny *(d)*, in spite of the fact that the latter were never exposed to a raised temperature.

It is expected that the unsophisticated layman is now ready to believe that the inheritance of acquired characteristics—in this case, the artificially produced blackening of butterflies—is conclusively proven by these experiments. Soon enough, however, opposition was voiced which necessitated later researches to find additional proof in a round-about way; but this, nevertheless, should be duly appreciated because it served to bring about a more intense study and a clearer conception of the subject. Ever and again it may be observed that the *pros* and *cons* of an argument are the more lively when deductions from natural science have a bearing on man. Never is the spirit of negation stronger than at the time when it is identical with the spirit of retrogression, stemming—consciously or unconsciously—human progress.

CHAPTER V

WAYS OF CHANGES AND INHERITANCE

IF we permit, by methods as described before, a living being (in our case, butterflies) to be influenced by warmth or cold, it may affect the generative organs (situated in pairs on the abdomen of the animal) in four different combinations (Fig. 2).

1. The exterior influence (in this instance, the temperature) changes only a limited part of the body (in our case, darkens the coloring of the wings), but leaves the rest of the body, and the germ plasm untouched (Fig. 2, C). This combination is by far the most frequent, and occurred in a considerable number of the butterfly experiments, thus facilitating at the same time the conclusion that surely not all, but perhaps only a few of the acquired characteristics, can be inherited or—to express myself more guardedly—can be traced in the next generation. That every acquired characteristic immediately becomes inheritable has never been insisted upon, even by the most enthusiastic disciples of this theory.

2. The opposite combination (Fig. 2, B), up to now has only rarely been observed; *i.e.*, the exterior influence changes only the germ plasm, leaving the rest of the body untouched. In Towers' experiments on the Colorado Potato Bėttle (*Leptinotarsa;* Fig. 4),

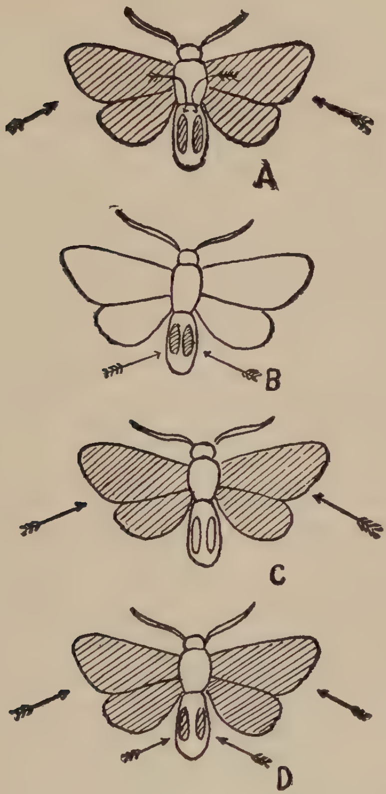


FIG. 2.—ACQUISITION OF CHANGES
(Four combinations)

A, Somatic Induction: Inheritance of acquired characteristics proper (transmission of a change from the body to the germ plasm). *B,* Influencing the germ without a change of the body. (This change, as a *"mutation,"* would suddenly appear in later generations.) *C,* Influencing the body without changing the germ plasm (*i. e.*, the acquired characteristic is not inherited). *D, Parallel Induction:* Pseudo-inheritance; after-effect. (Simultaneous direct influence on body and germ plasm)

(H. E. Ziegler)

we will soon enough come to know an example which has a bearing upon this question. In connection with the butterfly experiments mentioned so far, it may be appropriately commented that it certainly would have been quite important to breed a progeny from these specimens which, in the first generation, apparently remained unchanged. It is not at all impossible that the progeny of these unchanged specimens would have shown changes in spite of the fact that the parent generation had offered resistance to temperatural influence. A controlling test along these lines would have disposed of the frequently raised objection that here we do not observe a case of inheritable acquired characteristics, but rather that specimens were selected which right from the beginning seemed suitable.

3. The exterior influence changes the coloring of the wings; that is, influences, at first, a sharply localized and limited portion of the body. Here physical energy (temperature, etc.) is transformed into physiological energy; *i.e.*, the changes on the wings are passed on to the germ plasm, be it by way of the nervous system by metabolism and blood circulation (Fig. 2, A).

Ever since the question of the inheritance of acquired characteristics has been raised, one pictured the transformation of anything newly acquired from the body ("*Soma*") to the substance from which the progeny is derived (*Germ plasm*), in this way: The different strata of the body play the part of a mediator between the exterior and the interior world, between the space in which we live and the generative organ. Those strata play the part of a mediator not only because they are penetrable for physical stimuli,

but also because of their own functions. In principle, Charles Darwin (1875) included this process of transformation in his *Theory of Pangenesis,* and Detto coined the expression *"somatic induction"* for it.

4. Finally, the following combination seems possible, connecting combination 1 and 2 (Fig. 2, D): the same exterior influence which is directly noticeable by, for example, changes in the wings only, in reality similarly influences the whole body, including the generative organ. The same change, which on the wings manifests itself in its final form, is prepared in the germ plasm as an invisible determinant *("Anlage")* to be resurrected in the next generation. Such a process by Detto is termed *"parallel induction"; i.e.,* that which exteriorly develops in the body is simultaneously prepared in the germ plasm.

As far as our present object of study—the butterfly—is concerned, it is easy to understand that temperatural influences penetrate all strata of the body, and, in this way, may influence the germ plasm. The butterfly is a cold-blooded, or to be more accurate, poikilothermal animal whose body temperature changes with the temperature of its environment. If frost is in the air, the butterfly is very cold-blooded; if it is warm, the butterfly soon enough becomes "hot-blooded." Regarding all other influences of environment (light, humidity, food, push and pull, pressure, and cutting), H Przibram (1917) tried to prove that all these influences can reach the germ plasm through direct physical influence, and that this was even possible in warm-blooded animals (mammals and birds), even including changes of temperature, because the blood of warm-blooded animals also depends, to a certain

degree, especially when young, on the temperature of the environment.

The objection that it is not "somatic induction," but "parallel induction"—the direct influence on the germ plasm—has grown into the principal argument against the inheritance of acquired characteristics. The layman, of course, will ask: Is, by direct influence, inheritance not also brought about? Does the differentiation between 'somatic' and 'parallel' induction only amount to two different mechanisms or possibilities by which newly acquired characteristics are passed on to the germ plasm? Is not the final result the same and the positive one, too, as far as the inheritance of acquired characteristics is concerned?

CHAPTER VI

AFTER-EFFECT ONLY OR GENUINE INHERITANCE?

THIS question cannot be answered affirmatively without reservations. There certainly is a possibility that an exterior influence, which reaches the germ plasm, without the body playing the part of a mediator, may result in permanent changes ("mutations"). It is much more probable, however, that an exterior stimulus, influencing the germ plasm directly, results in temporary changes only ("modifications") which later on, in the grandchildren generation, again disappear in case the modifying stimulus influenced the grandparents only, but then was withdrawn by a return to normal conditions. This probability seems quite logical if one reasons as follows:

Let us consider the germ cells no longer as a part of the parental bodies, but rather as the first stage of the coming generation. That is, in the present case, we do not consider the egg as a cell in the body of the mother, but already as a very tiny offspring. Now, if some exterior influence endows this germ, or offspring, at the first stage of development potentially with the same characteristics which the mature organism of the parents actually shows, it stands to reason that this characteristic is not only newly acquired by the parents, but is also independently acquired by the progeny. If we were very accurate in the present case,

we should not use the term "inheritance" at all, because inheritance is brought about on the basis of an organic connection between parents and progeny. But in our case such a connection concerning the newly acquired characteristic is missing. In this respect, germ plasm and body, as far as this newly acquired characteristic is concerned, have nothing to do with each other and are entirely independent of one another. For this reason, we should not speak of anything else, but an "apparent inheritance"; or rather, should refer to it as an "after-effect."

This is more than just hairsplitting and a dialectic differentiation. Suppose (Fig. 3) that the parents experience a change of climate, the effect of which (for example, modification of colors) is also experienced directly by the germ plasm. But already at the time when this changed germ plasm matures into offspring, the temperature is restored to what it had been before. The previous change of climate will then have an after-effect upon the offspring which, for this reason, again show the modification acquired by the parents. But the germ plasm of the offspring will not be influenced by it any more, because the climate, meanwhile, has changed back to normalcy. The grandchildren will therefore develop into normal individuals, the acquired characteristics having been lost again.

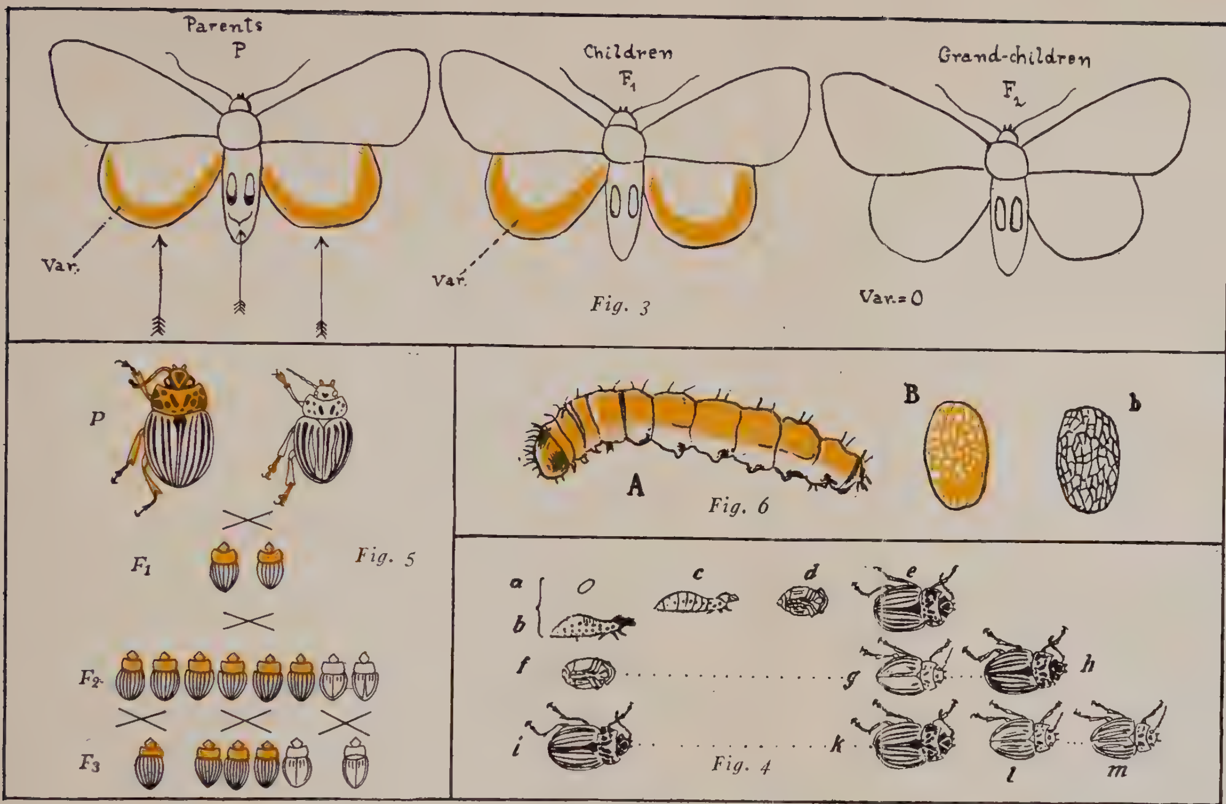


FIG 3.—LOSS OF AN ACQUIRED CHARACTERISTIC (*Var.*)

in the "grandchildren" (F_2) in case the changing influence (*arrows*) was only brought to bear on the grandparents (P) and directly from the outside on the germ plasm of the children (F_1): "After-effect" or "apparent (pseudo) inheritance"

(Original)

FIG. 4.—COLORADO POTATO BEETLE (*Leptinotarsa decemlineata*)

a, egg; *b,* normal larva; *c,* changed larva; *d,* chrysalis; *e,* normal beetle derived from the latter; *f,* influenced chrysalis; *g,* changed beetle derived from the latter (*var. pallida*); *h,* the unchanged progeny of the latter; *i,* influenced beetle which apparently (*k*) remained uninfluenced; *l,* the changed children and *m,* grandchildren

(Tower, 1906; Przibram, 1910 a)

FIG. 5.—COLORADO POTATO BEETLE (*Leptinotarsa decemlineata*)

Crossing the natural form (at the left) with the experimental form (at the right). P, parents; F_1, children externally identical to the natural form, bearing out the Mendelian "Dominance Rule" (alternative inheritance); F_2 grandchildren (three-quarters of the natural form, one-quarter of the experimental form, bearing out the Mendelian "Segregation Rule"); F_3, great-grandchildren (again proving "segregation")

(Tower, 1906; Przibram, 1910 a)

FIG. 6.—MOTH *Tineola biselliella,* CATERPILLAR AND EGG

a, Caterpillar colored by being fed with Suden red. *b,* Colored egg deposited by a moth developed from Sudan red-fed caterpillar. *c,* Normal, colorless egg

(Sitowski; Przibram, 1910 a)

CHAPTER VII

EXPERIMENTS ON BEETLES

LET us now study a case where the supposed "direct influence on the germ cells" was considered proven. According to Tower's own opinion *, this was the case regarding his breeding experiments with the Colorado Potato Beetle (*Leptinotarsa decemlineata;* Fig. 4). Tower availed himself of various methods to change the appearance of the Potato Beetle. Heat and aridity of air proved to be the most effective. Tower permitted these influences to make themselves felt only at strictly limited stages of development. As soon as such a stage of development was passed, the specimens were brought back to normal temperature and normal humidity.

If Tower permitted heat and aridity exclusively to influence already deposited eggs (Fig. 4, *a*), or larvæ (*b*), the latter, to be sure, underwent a change (*c*): instead of two parallel rows of black dots running lengthwise, bilaterally barred, there remained only

* Visiting American research institutions and also on occasion of a lecture I delivered in New York, I was told that Tower's experiments "were not considered dependable," but up to now I have not been able to ascertain the reasons for this opinion. On the contrary, I was assured that "Tower is a hard worker." Because substantiation is lacking, I see no reason to exclude Tower's experiments from this book. From personal experience, I know only too well that very often there is no tangible reason for inconvenient results to be "discredited" by fellow-scientists.

one, but the beetles emerging from these larvæ (*e*) are normal in appearance and so are the offspring.

If Tower permitted heat and aridity exclusively to influence the chrysalis (Fig. 4, *f*), the beetles emerged somewhat changed (*g*)—they were dwarfed and bleached (*Var. pallida*); but the offspring (*h*) of these bleached dwarfs again assumed the normal appearance of their grandparents.

If Tower permitted heat and aridity to influence the beetle in its stage of maturity only (Fig. 4, *i*), the beetle itself did not change any more (*k*), but the progeny were changed (*l, m*). The change observed in these offspring of dwarfed growth and bleached appearance is exactly the same as those changes brought about by a beetle emerging from a chrysalis, subjected to the same exterior influences.

These remarkable conditions of inheritance are to be explained through the relative maturing stages of the germ products: eggs and sperm cells of the beetle start to mature only after the beetle itself has completely matured, has achieved its full growth, and appears in the color scheme of the mature specimen. Previous to this time, the immature body proves itself sensitive to changes: not so the immature germ cells (Case No. C of the butterfly experiments, Fig. 2). After this time, the matured germ cells can be influenced, but not any more the completely matured unplastic body (Case No. B of the butterfly experiments, Fig. 2).

Concerning the solution of our problem, as far as these beetles are concerned, we are, to all appearances, faced with an exceptionally favorable division of "periods of sensitiveness." Either the body or the germ

cell is sensitive to changes, but never the two simultaneously at the same period of their development. If the germ cells can be changed while the body is no longer plastic, this body could not have played the rôle of a mediator between environment and the germ plasm. A change that was not acquired by the body cannot be passed on to the germ cells. For this reason, the germ cells must acquire the changed characteristics independently from the body.

Now, is a direct influencing of the germ cells proven by this? Not at all convincingly! Because those parts of the body which show the changes in question (head, pro-thorax, and the stiff wing covers) are rigid, unplastic products of the skin below (*hypodermis*), are products of the body with no more life of their own—in this respect comparative to our hair and nails. To insist that these rigid parts prove that the body is unchangeable amounts, according to Semon, to exactly the same thing as if one says that man is not sensitive to joy and pain as soon as a rigid mask hides the expression of his face. The living soft skin on which the horny (chitinous) covering of the beetle rests may very well remain sensitive—even on the mature body—to exterior influences.

To express it more clearly: While the visibly "dead" outside parts of the body cannot be influenced any more, an influence may still make itself felt on the parts immediately beneath the horny surface; and it would be these parts, then, which would relay the respective influence (as "somatic induction") to the germ plasm.

That dwarfed and bleached experimental forms of the Potato Beetle are genuinely inheritable, and appear

not only in the next, but also, in succeeding generations, is clearly proven by Tower's continued breeding experiments. Even though heat and aridity apparently had no influence on the mature beetles, their offspring (Fig. 4, *l*)—and not only they, but their offspring's offspring (*m*), and all the following generations—are changed according to the intention of the experiment. Here we have a genuine effect of heredity which could hardly be borne out with the assumption of only a direct influence on the germ cells, and be disposed of as just an after-effect. Additional material is derived from the fact that experimentation specimens when crossed with normal specimens (Fig. 5) were subject to the Mendelian rules which, according to Johannsen and others, is to be considered as a proof of genuine inheritance.

The question whether we are dealing here with an influence which is direct or indirect (*i.e.*, transmitted from the body), influencing the germ cells can hardly be decided unequivocally in favor of direct influence by Tower's experiments. We will leave it to breeding experiments, to be mentioned later on, to make things clearer in this respect, and will now deal with other objections generally raised against the inheritance of acquired characteristics.

CHAPTER VIII

ATAVISMS OR NEW CHARACTERISTICS?

THE value of a number of such experiments, which tests and results seem to substantiate the inheritance of acquired characteristics is diminished, regarding the positive weight of proof they carry with them, for the simple reason that Weismann (1904) and H. E. Ziegler insist that not a newly acquired characteristic is to be observed here, but rather an old one—one which for generations had been dormant, and now has been resurrected. In short, we are not dealing here with new acquisitions, but rather with atavisms.

Headed "Back to the Jungle," The *Daily Express* of London, under date of May 2, 1923, published an interview with Sir Arthur Keith, according to which the latter claims that all "acquired" characteristics are truly nothing but "lost" characteristics; that we are not able to produce in our breeding experiments new adaptations, but could go only so far as to eliminate prevailing adaptations. This theory applied to man would mean a retrogression into primitive conditions, the retrodevelopment of civilized man into a savage, which is exactly the contrary condition to that which inheritance of acquired characteristics would bring about, and which, considering the possibilities this opens in the realm of productive eugenics, is a goal greatly to be desired.

Just to illustrate how and in which instances this objection of atavism is often raised, I refer to my (Kammerer, 1909; 1910 c, e; 1911; 1913 a; 1914; 1919; 1923 a) breeding experiments with the Western European Midwife Toad (*Alytes obstetricans*). To make things plainer, the method of propagation generally prevailing among the frogs and toads (Fig. 7) should be mentioned here. These frogs and toads deposit small eggs by the hundreds. They are covered with a transparent, gelatin layer which swells enormously (Figs. 7, 1) as soon as the eggs touch the water. The newly emerged young, the tadpoles (larvæ), as a rule have no special organs of respiration, but they soon grow outer gills (Fig. 7, 3), which later on atrophy, and are replaced by inner gills (Figs. 7, 4). For quite a number of weeks the tadpole has no limbs; then the hind extremities appear, followed by the forelegs; the tail atrophies, and the small developed frog changes his abode to *terra firma.*

CHAPTER IX

EXPERIMENTS WITH THE MIDWIFE TOAD

DIFFERENT from the propagation method described above, the Midwife Toad does not deposit her eggs in the water. This species produces only eighteen to thirty-eight eggs, which are comparatively large, as they contain a generous quantity of yolk. These eggs, which form a chain, are deposited on land where the gelatin-like layer, which covers them, has no possibility to absorb water and swell up. The male pushes his hind legs through the string of eggs (now one tightly packed ball), and carries them like very tight shackles around the upper part of the hind legs (Fig. 8), until the tadpoles emerge from the eggs. These young are still in a stage where they are without legs, but they have already developed the inner gills. That stage where they are without gills at all, and where they have only outer gills, they have already passed inside of the egg. From now on, the development of the young Midwife Toad is analogous to that of other toads and frogs: first, they are two-legged creatures; then, four-legged; next, the tail atrophies; and after this, the fully developed little toad changes his abode from water to land.

Regarding the propagation and development of the Midwife Toad, I am now going to mention only two of the changes which I forced, by experimental breed-

ing, and which seem very well suited, by merit of contrast, to throw a light on the much discussed problem of atavism. The first change led away from the water; that is, the tendency of the Midwife Toad to become independent of the water was accelerated to its ultimate consummation. If the development of the egg is speeded up through heat, and if at the same time care is taken to slow down all those movements which tend to facilitate the emerging of the tadpole from the egg, by subjecting the eggs to relative aridity and darkness, one succeeds in developing gigantic eggs, which are then burst by the tadpole only when it has already grown hind legs. These eggs and tadpoles produce dwarf-like toads which now, from generation to generation, produce eggs that are proportionately more limited in number, but are larger and larger, and more and more rich of yolk. If the environment continues to be warm, rather dry, and quite shady, tadpoles emerge from these eggs with completely developed hind legs. If restored to normal conditions, tadpoles are produced with just the beginnings of the hind legs.

The second change leads back to the water; that is, that degree of independence from the water already acquired by the Midwife Toad is nullified. If mature Midwife Toads are kept at a temperature of 77° to 86° F., they abandon entirely their normal mode of propagation. Unaccustomed to the heat, they dry up and are forced to take to the water for the necessary humidity. Here the sexes mingle, and the mating period is passed in the water, and there the eggs are deposited. At the very moment, of course, when the gelatin-like cover of the eggs comes in contact with

Fig. 7.—DEVELOPMENT OF THE FROG (*Rana*)

1, Freshly deposited eggs; below them eggs with their gelatine-like layer swelled. 2, Embryos shortly before emerging from the egg in two different stages (*a* and *b*). 3, Tadpole (*larva*) with outer gills, without legs. 4, Larva with hind legs; the forelegs (*v*) still imbedded below the skin; *n*, nostrils; *o*, operculum; *p*, lungs
(Pfurtscheller)

the water, the cover begins to swell, and they lose their stickiness. In this case, of course, they do not dry up; so, later on, they cannot close in tightly around the thighs of the male, in this way making it impossible for the male to fasten the string of eggs to his hind legs. Accordingly, the eggs remain in the water; but, in spite of this, sometimes a few of these eggs, of one or the other string, produce tadpoles. These are at a stage of development where they still have outer gills of which the Midwife Toad has only one single pair (instead of the three pairs which tadpoles of other frogs and toads possess). Mature toads grown from such tadpoles are unusually large.

"Water-eggs" of later generations obviously become poorer of yolk, and therefore smaller; but the gelatin-like cover becomes thicker and thicker. Tadpoles from "water-eggs" of later generations show a gain of dark coloring matter, and a progressing loss of yolk, until the vitelline-(yolk-) sac ultimately vanishes altogether. Changes regarding the gills are also observed—they become shorter, simpler, and appear to be coarser. While usually only the first of the branchial arches of the skeleton are possessed of gills, in the great-great-grandson generation all the three gill-arches have grown gills.

Possibly to be better adapted to the more difficult seizing of the female in the water, the male of this, and, to a certain extent, the male of a previous, generation also develop a rough, blackish nuptial pad (Fig. 9 right), on their fingers and forearm. Besides, the muscles of the arm are strengthened, which in turn results in giving the fore limbs a more converging

position. All these are exterior sex characteristics, to be found in all frogs and toads, which mate in the water, but are ordinarily not to be observed on the Midwife Toad, which normally mates on land.

It is easily understood that the last related group of experiments are confronted with the objection: If the Midwife Toad, which takes care of its eggs, and which, for the period of mating and embryonal development, has evolved so as to be independent from the water, returns to the moist element, we observe only a retrogression to old methods of propagation, and not a progression towards new. Weismann (1904) and Schaxel (1913) raised this objection, but both scientists omitted to pay any attention whatever to the first mentioned group of experiments, even though it ought to be clear enough that here we are surely not dealing with an atavism.

If one admits the atavistic character of the second group of experiments ("back to the water," renouncing the care of eggs), one absolutely cannot avoid admitting that the first group of experiments (gigantic eggs, prolongation of post-maturity on land) amounts to a genuine new acquisition. In the first instance, the whole embryonal and post-embryonal (larval) development is passed in the water, even including the actual fertilization of the eggs. In the second instance, a development more prolonged than under normal conditions is passed outside of the water. The stage of living in the water commences, just as under normal conditions, at the moment when the tadpole emerges from the egg, but the latter leaves the egg in a far more progressed stage of development.

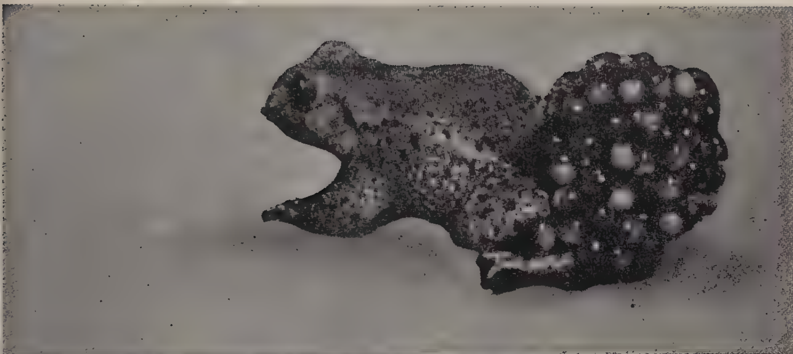


Fig. 8.—MIDWIFE TOAD (*Alytes obstetricans*)

Male, with tightly packed eggs around the thighs
(From a living specimen; photo by Walter Köhler)

Fig. 9.—TWO MALES

left, normal specimen as kept for controlling purposes. At the right, specimen of the fifth tion bred in water. The dark shading of the left hand denotes the extension of the "nuptial On the right hand, there is a small callosity below the thumb due to the skin of this hand been removed for microscopical investigation, and when regenerating did not attain its former extension

(Original photo by Stewart, Cambridge)

From this stage of development it does not take as long as before to change again into a land toad.

If the development in the one direction—"back to the water"—constitutes retrogression, the development in the other direction—"away from the water"—can mean nothing but progression.

CHAPTER X

WHAT ARE "HEREDITARY CHARACTERISTICS"?

AGAINST this logical, unassailable answer, Baur (1913), of course, holds yet another objection in readiness. He contends that the essence of an "hereditary characteristic" is not at all exhausted with what we just see before our very eyes. According to Baur, not that which is visible is hereditary, but the faculty to react to exterior influences sometimes in this, sometimes in that way. If we apply this to our example, the hereditary characteristic of the Midwife Toad would consist not in the fact that the Midwife Toad takes care of its eggs, nor in its failure to do so, nor in the longer or shorter embryonal development, etc., but in the faculty (according to dryness or humidity, living on land or in water) to develop, either with or without taking care of their eggs, and sometimes a longer and sometimes a shorter stage of development to be passed within the egg.

Baur doubtlessly hit the mark in explaining his conception of "hereditary characteristics" in general. But Baur's opinion is surely too far-reaching in his extension of the definition to those cases where, in one direction or in the other, the development so greatly deviates from the normal, as in the case of the Midwife Toad. Baur, himself, imagines the "reactionary normalcy"; that is, the faculty of an organic character-

istic to adapt itself to the demands of the respective situation around a fixed center, as something very definite and limited. He arrived at this opinion after having made seed experiments with beans which—depending whether they matured under favorable or unfavorable conditions as to their nourishment—are subject to certain differentiations in size, about from seven to fifteen *mm*. It makes no difference whether a small or large bean is picked for the purpose of raising seed; the new crop will again consist of beans from seven to fifteen *mm*.

Of course, what holds true regarding comparatively small differences as to the size and weight of beans, should under no circumstances be generalized in regard to developments so contrasting that they change the whole picture of the species, developments which were not limited to the one characteristic from where the breeding experiment started, but which in the course of the breeding embraced almost all the other characteristics in all the other stages.

If Baur had tried to raise crops of his large beans under constantly favorable conditions, or raise small beans under constantly unfavorable conditions, he most probably would have found—provided he finished the experiment eventually under the old conditions—that, in both cases, he had produced differentiations quite distinct from the original deviations from seven to fifteen *mm*. In the first case, it was to be expected that the smallest, in the second case the largest, sizes would be eliminated, to be most probably replaced by more distinct extremes; in the first case, even larger; in the second case, even smaller sizes of beans.

CHAPTER XI

THE CONTROVERSY ABOUT THE INHERITANCE OF ACQUIRED CHARACTERISTICS

For the time being, the fact that the initial differences (brought about by the experiment with the Midwife Toad) in the course of time embrace all the characters of the species, tended to lend new arguments to Baur's objections. As has already been stressed, regarding the experiments on butterflies, the way to test the inheritance of acquired characteristics is to deprive the later generations of the exterior influences which brought about the changes in the earlier generations. Baur (1914, p. 57) insists that this restoration into normal conditions of life of the species was impossible, as far as my experiments with the Midwife Toad were concerned. When, for example, the tadpoles prematurely emerge from the egg and seek the water, a deviating condition has already been established which, of course, must of necessity result in a whole chain of further deviations in every generation, without the necessity of this deviation being passed on hereditarily.

Even before this objection was put black on white, it was disposed of by two controlling tests which I had performed and reported on, namely:

1. Eggs of "normal" Midwife Toads which were taken from the hind legs of the male, when they ma-

tured in the water and prematurely threw off the cover of the egg, produced specimens which later on raised their young in true midwife fashion.

2. Eggs of "abnormal" Midwife Toads (that is, such toads as did not take care of their eggs any more, but simply deposited the eggs in the water) produced specimens in which the instinct to attach the eggs to their thighs was lacking, even though these specimens themselves had passed their period of development on land.

In spite of the fact that Richard Semon (already as far back as 1912, p. 156) had taken occasion to direct attention to these controlling tests of mine, Baur repeated his objections, without modifying them, in a new edition of his book on inheritance, and only in the very last editions were these objections eliminated and replaced with an almost contemptuous, grand gesture of dismissal in the introduction.

How nonsensical the last mentioned objection is, aside from the controlling tests which actually disprove it, becomes even clearer in the light of the following:

If, in the course of a change influencing instincts and development, it has become manifest that eggs are deposited in the water instead of on dry land, and that out of the eggs deposited in the water emerge tadpoles not quite as developed as under normal conditions, then this and other changes no longer belong within the realm of experimental conditions, but already belong to variations brought about by them. Now, quite a number of characteristics of the organism are closely connected with each other. If only one of them is changed, all the others are influenced in turn, and are also forced to undergo a change. If now the first

changed characteristic which influenced all the other changes is proven to be hereditary, we must regard the whole complex brought about by the first change as hereditary.

The most important variation in the case of the Midwife Toad is the voluntary relinquishing of carrying the eggs and taking to the water at the mating period, even after the influence which brought about these changes of propagation has again been eliminated. The unassailable proof of genuine inheritance was brought about here by the aforementioned controlling tests and strengthened by the fact that, in crossing "abnormal" Midwife Toads with "normal" ones, the hybrids are subject to the Mendelian Rule.

On account of his different opinion, Baur jeopardizes his own definition of what characteristics really constitute and confounds action and reaction, changing influence and influencing change. Suppose there were a correlation between the green color of the tree frog and his ability to climb, or the development of the suction cups on his fingers and toes—which is not so impossible at all—Baur, in his way of thinking, might deduce this conclusion: because the suction cups already develop at the tadpole stage when the immature frog has only two legs and is not, as yet, of green color, the subsequent development of the green color is no hereditary characteristic whatever. As plainly absurd as such an objection may be, through remonstrances like this of the opponents, all of the facts ascertained in the realm of inheritance have quite often been just as thoroughly turned upside down.

Among the numerous variations which gradually developed in the course of my experiments to change the

Midwife Toad into a water-loving frog—variations which are the reasons for the state of confusion into which Baur drifted—none commanded more attention than the development of the nuptial pad on the male, at the period when he is eager to mate (Fig. 8). Even though admittedly an atavism and not a new acquisition, the development of the nuptial pad constitutes a valuable addition to the problem of variations and to that of inheritance, for the reason that the nuptial pad has never been observed on the Midwife Toad when at large.

Only on one specimen of a related species with a habitat in Spain (*Alytes cisternasii*) Gadow observed a small, horny spot on the tip of the thumb; but it seems more probable that this did not constitute a nuptial pad, but rather was engendered to assist in digging deep, subterranean holes in which the *Alytes cisternasii* prefers to live.

In a letter to the author, dated February 25, 1924, M. Perkins of Cambridge (England) states: "A little while ago I had an opportunity of examining the *Alytes cisternasii* in the British Museum. . . . I saw that, as Dr. Gadow had observed, the tip of one finger and also the next to it was brown and horny in the old males. . . . Also it seemed to me exactly like the corns and horny patches which may appear on the palmar and plantar tubercles of old toads, because it was not black but brown and it was without the least trace of spines, therefore I concluded that it was a modification in connection with the creature's digging habits like the 'spur' of Pelobates. Because the British Museum specimens were few in number and there was not among them a male in the breeding

season, I wrote to . . . the 'Museo Nacional' in Madrid (as you suggested to me) and asked what was known in Spain of the 'nuptial pad' of *Alytes cisternasii.* The reply has just now arrived from Dr. Ernesto Cusi . . . Mr. Cusi also sent me specimens that I might make sections, six males (four of them bearing eggs) and three females, even the females have the 'nuptial pad' . . . well developed! I send you photographs of them, and will later on send some of the sections when they are ready."

Unfortunately, we are only rarely in a position to produce in the laboratory characteristics, or change established characteristics which, under identical conditions, could not also be brought about in Nature. Nature, however, has anticipated most of the experiments of the research worker. The already mentioned blackening of butterflies is well known to collectors of such, according to warm and cold seasons and warm and cold climes. The bleached form of the Colorado Potato Beetle, which Tower achieved by artificial heat and aridity, is frequently observed in desert-like areas. The change of colors in salamanders, about which more will be said in Chapter XVI, permitted me to exceed to a small degree only those changes which are naturally brought about by different colors of different soils. The small degree I achieved is nothing more than intensified changes as brought about by Nature on the same species, and not at all new, never-to-be-observed-before in Nature and strictly limited characteristics.

But such a never-to-be-observed-before in Nature characteristic is the atavistic nuptial pad of the Midwife Toad (*Alytes obstetricans*), and for this reason

Fig. 10

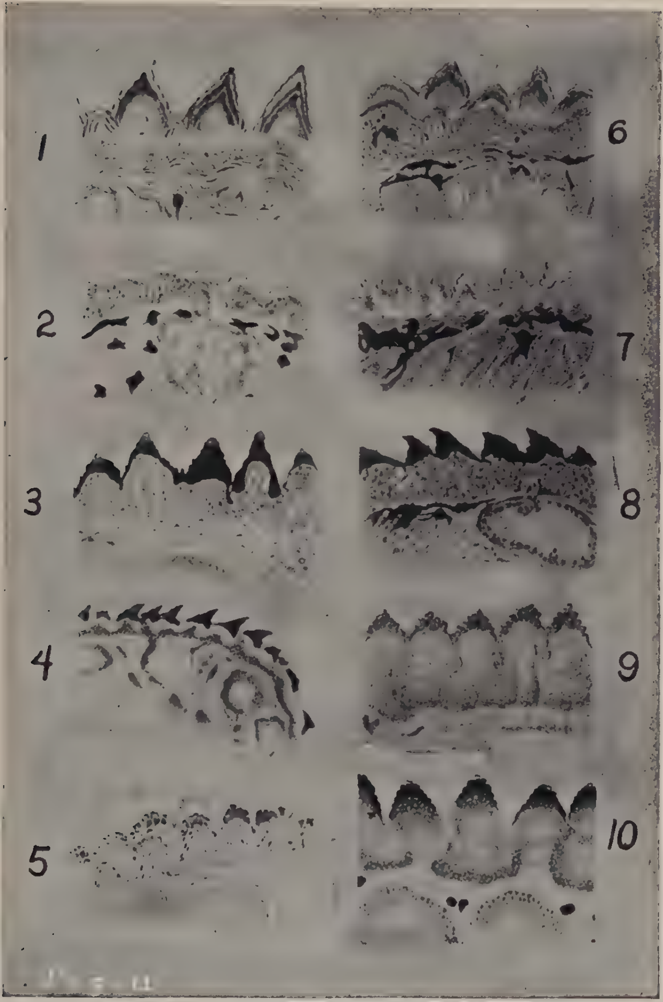


FIG. 10.—THE MIDWIFE TOAD (*Alytes obstetricans*)
(Microtome sections)

At the left section through the "pad" of a normal "land-bred" specimen. At the right section through the nuptial pad of a male of the fifth "water-bred" generation. While the right picture shows callosities, the left picture does not show any in spite of being magnified
(Original photo by Stewart, Cambridge, England)

FIG. 11.—NUPTIAL PADS OF EUROPEAN FROGS AND TOADS
(Microtome sections)

1. *Rana temporaria=fusca*
2. *R. agilis=dalmatina*
3. *Discoglossus pictus*
4. *Pelodytes punctatus*
5. *Bufo calamita*
6. *R. arvalis=oxyrrhina*
7. *R. esculenta*
8. *Bombinator igneus*
9. *Bufo vulgaris*
10. *Bufo viridis=variabilis*

there were many attempts simply to deny this phenomenon. It was W. Bateson especially (1913, p. 202; *Nature,* 1919, p. 334: *Nature,* 1923, pp. 391 and 738) who played a stellar rôle in this question. Indeed, he did his very best to maintain his contention that the nuptial pad was no nuptial pad at all, but just a spot of black pigment. At some other time, he insisted that the pad was nothing but a callosity without the characteristic rugosities of a nuptial pad. Bateson also tried to explain the pad as an accidental deformity or just a shadow on the photograph, and finally even went so far as to insinuate that the pad was brought about by retouching the picture. Bateson came to the last mentioned conclusion because some dirt sticking to the small finger of the frog was not retouched, as I did not want to tamper with the photographic document in any way. Because the nuptial pad only very rarely extends to the small (outermost) finger, the botanist Bateson blames me, the zoölogist, for not knowing my very own branch of science and insists that, on account of insufficient zoölogical knowledge, I caused the "touching up" of the wrong finger to prove my contention. When in 1919 I published microtom sections of the pad formation (Fig. 10), Bateson raised the objection that the sections were not of the Midwife Toad, but were derived from some other genera. And when it finally became clear that these sections were greatly different from sections through pad formations of other European frogs and toads (Fig. 11), Bateson insisted that the section derived from the Midwife Toad did not constitute sections of the tissue of a genuine nuptial pad.

To dispose of these absurd attacks which contra-

dicted themselves and tended, in a way, to besmirch my standing as a scientist, I went to England in May, 1923, in response to a cordial invitation from the Natural History Society of Cambridge, and took with me a specimen of the male Midwife Toad, which had developed a nuptial pad, together with a number of microtom sections of the pad. The genuineness of my specimen and the correctness of my contentions were verified by many scientists, among them MacBride, Gadow, and E. G. Boulenger. Subsequently, M. Perkins (in *Nature,* 1923, p. 238) proved that the nuptial pad of the Midwife Toad is distinctly different from the nuptial pad of other frogs and toads, as is clearly proven by the chart compiled by Lataste (Fig. 10); but its extension on the body and the composition of its tissue mostly resembles the nuptial pad of those frogs and toads which are most closely related to the Midwife Toad, that is, the *Discoglossidæ* to which family the Midwife Toad belongs.

Even though I could not bear to have the nuptial pad I raised on the Midwife Toad taken away from me, so to speak, I do not stress its existence to prove the inheritance of acquired characteristics. In the first place, the objection that it is an atavism may justly be raised against it; and in the second place, the objection of a direct influencing of the germ plasm could seem justified, because the changes eventually leading to the formation of the pad were originally brought about through heat as a stimulus. As the Midwife Toad is a "cold-blooded" (*poikilothermal*) animal, the heat could directly penetrate the whole body and reach the germ plasm. To be sure, it is quite improbable that the simple stimulus of heat (which only by

way of complicated instinctal and other changes resulted in pad formation) could bring about this development in the germ plasm, including the entire chain of variations. Be that as it may, there exists such a wealth of evidence regarding the inheritance of acquired characteristics, that we need not lose time with a proof against which only a shadow of an objection could be raised.

CHAPTER XII

DIRECT ADAPTABILITY OR SELECTION?

JUST as if this whole conglomeration of main and minor objections, which have been dealt with so far, were insufficient, one more crucial objection raised by the opponents of the inheritance of acquired characteristics has yet to be taken care of.

As the last related case of the nuptial pad illustrates, with tolerable probability, many acquired characteristics are also of a practical nature; they are "adaptations." And now these "direct adaptations" are not supposed to be adaptations to the environment! They are not supposed to be acquired on account of direct connection with environmental influences, but are supposed to be brought about indirectly by selection.

With this, they would be removed from the realm and the conception of "acquired characteristics," because these are brought about by conditions of life, that is, exterior influences. Selection, however, brings about—if it brings anything about at all—adaptations only by way of inner influences of the living being. To explain this and to simultaneously show how the objection of atavism and selectionism very often work together, we may with advantage draw again on the experiments of butterflies, as related in Chapter IV, which were conducted by Standfuss, Fischer, and

Christian Schroeder. These experiments with butterflies had to weather a similar storm of objections.

These objections culminated in the assertion that the color changes of the butterflies were not effected by keeping the experimentation specimens in a refrigerator, but that the progenitors of these butterflies, living at the glacial period, had already been dark colored. That sounds like a vague, unprovable assumption; but, to a certain extent, it is substantiated by facts ascertained by W. Schuckmann. The latter discovered that normal butterflies, which were not influenced by frost before they emerged from the chrysalis, are of a darker color than in the ensuing stage. We know that, in its main stages, the development of an individual repeats the stages of evolution in general. For this reason, one could assume that a stage shortly before the emerging of the mature butterfly from the chrysalis corresponds to a condition at which, "once upon a time," the butterfly was already winged. As the stage of the darker coloring of the butterfly just precedes the present stage of a fully developed butterfly, one could assume that the stage of "once upon a time"—cosmologically spoken—was not so very long ago. It seems not at all far fetched to assume that the last glacial period could be held responsible in this respect, and it is not at all impossible that that which artificial frost brings about today, could have been brought about by a natural low temperature before.

Two new questions arise here:

1. Would not the acquisition of hereditary blackening, even if the explanation given above is correct, simply be placed into a previous geological period

only? There is no doubt that the blackening had to be acquired once, and if not in the modern refrigerator, then at a time when the wide and open spaces were still covered with glaciers, and when the average temperature was a few degrees below today's average temperature. Be that as it may, then or today, the darker color was an acquired characteristic which was passed on hereditarily.

2. Supposing that the darkening of the color brought about in our days by artificial frost constitutes an atavism from the glacial period, a corresponding darkening of the color brought about by heat could not very well be termed an atavism.

Let us answer the second question first. What are the results of these extreme temperatures? They check the development of the butterfly; they conserve a stage which usually is only a way-station, and which precedes the stage of maturity. Very often this checking can no longer be caught up with; the butterfly emerges in a dark-colored, *i.e.*, in a relatively immature condition. As a matter of fact, the butterfly is not actually darkened, but rather has not, as yet, reached the stage of a lighter coloring, because the dark stage is the primary stage, whereas the lighter stage is the secondary. But then it would not make any difference whatsoever by what influence the primary condition, *i.e.*, the secondary (final) condition at a previous cosmological period, is definitely fixed once more as final condition. Carbonic acid and other poisonous vapors, according to M. v. Linden, affect the butterfly in the same way, provided these agents are brought to bear upon the butterfly at the time when they may result in a checking of a complete de-

velopment. For this reason, butterflies that pass the chrysalis stage in the neighborhood of smoke-stacks look as though they had been discolored by soot, but the latter has absolutely nothing to do with the darkening and only proves that the butterfly emerged from the chrysalis at a premature stage which usually is still passed within the cocoon.

Why should not heat have the same effect? Heat does not work as such, but only as a means of checking the development of the butterfly at a time when it is not yet supposed to emerge from the chrysalis. Heat is not at all necessary to bring about a darkening of the colors, because the darker coloring at this stage has been already prepared since glacial times, as a part of the normal development of the chrysalis. Frost, heat, carbonic acid, etc., today play only the part that "once upon a time" was taken care of by conditions prevailing during glacial times.

And now, back to our first question: Was the hereditary characteristic of the darkening of the colors acquired at the glacial period? Certainly not on account of direct contact with the cold, is Weismann's opinion; but darker colors must have been of advantage to the butterflies of the glacial days, because darker colors absorb more heat than lighter ones. Among a number of otherwise entirely similar butterflies, there are always "accidentally" a few of darker and a few of lighter color. In cold weather, the darker have a decided advantage over the lighter ones which very often freeze to death, whereas only the darker ones are left to propagate themselves. The latter pass on their inborn dark colors to their progeny, and of their progeny those again are sifted out which are the dark-

est, and so forth. Gradually, in this way, there is bred the most intensive degree of dark coloring that can be reached by this certain species of butterfly. Incidentally, the degree of most intensive dark coloring, by absorbing most of the life-lengthening warmth of the sun, tends to conserve the species.

The explanation on the basis of selectionism is not exhausted with diluvial-glacial darkening, but is also applied to the means and methods in which Standfuss, Fischer, and Christian Schroeder conducted their blackening experiments. As already mentioned in Chapter IV the majority of the butterflies raised in the refrigerator were darker than normal. The not inconsiderable remainder, however, had succeeded in defying the checking influence of the cold, and reached the final "light" stage. For breeding purposes, of course, none of these specimens which had defied the intentions of the experimenters were used, but those which had developed dark. Small wonder it echoes from the camp of the selectionists that these dark specimens again bred an also dark progeny. Were the specimens to be used for breeding purposes not selected? And to top this all off, not only a characteristic was chosen which was brought about by temperatural experimentation, but one which already at the glacial period had developed by selection! It was only left to the present-day experiment to continue, by unintentional artificial selection, that which natural selection had already begun long ago.

This is Weismann's objection, raised on the basis of selectionism, which in Chapter V made me wish that the normal-colored butterflies also should have been used to continue the breeding experiments. Cer-

tainly, if the progeny of these normal-colored butterflies would have turned out to be darkened, the objection raised on the basis of selectionism could have been dismissed. But with the same apparent justification which, in connection with Tower's experiments on the Colorado Potato Beetle mentioned in Chapter VII, could not stand close scrutiny, here again the objection could promptly have been raised of a direct influence on the germ plasm. A changed progeny without the parents being changed? That is, of course, only possible in a case when the germ plasm has been influenced without the necessity of the body to play the part of a mediator.

CHAPTER XIII

A PROBLEM MADE INSOLUBLE

As is clearly shown here, the problem seemed very well on the way to be stifled once for all. There hardly remained a single possibility to prove heredity of acquired characteristics. We never know exactly what a live being looked like a few thousand years ago. Every apparent "new" characteristic may already have lain dormant in the creature!

Moreover, every characteristic could have been engendered by selection, that is, by natural selection before the latter was resurrected, by way of experiment. Such reasoning seems to fit remarkably well, especially when the characteristic amounts to an improvement. If this is not the case, there still remains the argument to be advanced that even though we do not clearly see what it is good for, the characteristic certainly serves the one or the other purpose hidden from our eyes.

Now, the way the opponents of the inheritance of acquired characteristics would continue their arguments would be about as follows:

Why, yes, a characteristic may just as well have been developed, independent from any purpose, in the course of the experiment by artificial selection. The latter is always practiced, because we unconsciously choose those specimens which seem most fitted to achieve the desired purpose of the experiment. To

be sure, it is quite remarkable that for experiments destined to perfect one another and begun with material of the same kind, but with different aims in view, for example, the case of the Midwife Toad in the direction of an intensified or a diminished dependency on the water, always such specimens are picked as are just wanted to achieve the one or the other result.

But such trivial matters fail to feaze the opponents of the inheritance of acquired characteristics. They stand ready with a whole arsenal of other objections like Baur's conception of characteristics which means the same as A. Lang's "transgressive œcologisms," atavism, after-effect, etc.

Finally, every new characteristic, even though new, may be the result of a "mutation" by direct influence on the germ plasm. That does not only apply to temperature and regarding "cold-blooded" animals, because "warm-blooded" animals also (mammals and birds), especially as sucklings and fledglings, but also even when mature, are not as normal-blooded as has been generally assumed. Thermometrical observations by Sumner on mice, Przibram (1917, 1923 b), Bierens (1922) and Congdon on rats yielded the information that a change on the outside temperature of about 54° F. amounts to an internal change of temperature of about 2° F. This leaves enough room for action to give rise to the slight possibility of a direct changing of the germ plasm within the scope of temperatural experiments on heredity.

Aside from temperature, other influences may reach the germ plasm such as humidity and density of air, light, electrical, chemical and even mechanical influences. All these may reach the germ plasm by means

of physical action without the necessity of a detour of a physiological reaction. Without being an opponent to the inheritance of acquired characteristics, H. Przibram (1917, see Chapter V) plainly shows how far one may go in this assumption. Even in cases of mutilation, the germ could be deprived of substances necessary for the restitution of the lost organ whereby an exactly similar result could be effected on the progeny. Let us draw on Schroeder's experiments on certain caterpillars of moths (about which more will be said in Chapter XXXIII). Schroeder forced a change of instinct on the caterpillars, effecting that the latter, instead of using the point of the willow leaves they live and thrive on, used the edge to wrap themselves up in. One could now reason that chemical differences of the cellular texture of the point and the edges of the leaves reacted on the germ plasm in such a way as to be resurrected as a change of instinct in the next generation.

The unscientific substantiation of this opposition to the inheritance of acquired characteristics is based on two things.

1. In the foregoing, it has already intrinsically been shown how all objections are raised in such a form as to make it impossible to answer the question altogether. The problem as such, being removed from the possibility of solution, by this has naturally and incidentally been put outside the pale of exact science. And this is not saying too much because if one nowadays talks to a genetician about the inheritance of acquired characteristics, the latter will compassionately shrug his shoulders. Such a problem does not exist for him. He would not take himself seriously any

longer nor would his colleagues if he would dissipate the gift of his scientific reasoning on a question definitely decided long ago (an observation also made by Redfield, 1923). Indeed, it would be a pity to lose time with this question because every attempt to solve it right from the beginning is sacrificed to the same objections:

The germs can always be reached directly.

No new characteristic needs to be really new, but may constitute an atavism.

Each and every new characteristic may be effected or intensified by selection, as far as they resulted from experiments, even without being of great duration.

I found myself face to face with this insurmountable wall, and resignedly abstained from breeding further generations whose scientific value in every case would have been depreciated. I developed the atrophied visual organ of the blind and bleached newt *Proteus* (Fig. 39, Chapter XXVII), which lives in subterranean caves, into a seeing eye. But of what avail would it have been to follow up this certainly gratifying result in the progeny? All the objections usually raised would have fitted this case:

Hardly experiencing any resistance whatever, the light penetrates the transparent body of the *Proteus* and reaches the generative organs. The *Proteus* comes from forebears seeing and living on the face of the earth, and did not, therefore, acquire anything with his newly developed eye, but merely regained that which it had formerly possessed and which was securely retained in the germ plasm. Finally, it is very practical to be able to see when there is light; functioning eyes are of value in selection. Not the light

itself, but selection restored the faculty of visual perception to the atrophied eye. For all these reasons, I was satisfied with the first and only generation.

2. On the other hand, in this way the researching scientist right from the beginning is handicapped in proving that acquired characteristics are hereditary; on the other hand, the whole burden of proof is laid upon his shoulders, every attempt of proof being met with the objection that the thing could be entirely different in one respect or another. But for that which is nothing more than just a different interpretation, the substantiating proof is again placed upon the shoulders of the researching scientist. In this way, the latter is frequently forced to unnecessary detours; the opponent of the inheritance of acquired characteristics does not need to prove his case. His interpretation, be it ever so inadequate and unsubstantiated, is immediately accepted with decidedly more readiness than any proof substantiated by facts which are cited as evidence for the inheritance of acquired characteristics. It is an old story that it is much easier and cheaper to substantiate negative assertions than positive ones, especially in times when it so happens that the negative interpretation is just in fashion. This is the case just now regarding the inheritance of acquired characteristics; but already there are unmistakable signs of an approaching change.

3. Intimately connected with the unscientific procedure of the opponents who always insist on proofs only, as far as the champion of the inheritance of acquired characteristics is concerned, the following method is quite often to be noted in this respect:

To weaken the proof of such manifestations as may

be observed by everybody, invisible processes are cited which are claimed to work mostly in the chromosomes (Chapter I) of the cell nucleus. For example: Weismann assumes for his mysterious "germ plasm" within the cell nucleus an improbable, complicated construction which remains beyond the limits of perception and compared to which the atom-and-electron-theory appears as child's play. The shaking-up of this complicated edifice, which originates only by interior rearrangements independent from exterior "shakeups," is drawn upon as the one and only origin of all organic changes and of all organic progression. For the sake of this uncontrollable, interior world all signs that the exterior world makes itself felt there and remains effective must be pushed aside. Based on this inner cosmos of his own concoction which marks the pinnacle of inconceivability, Weismann insists, leaning on his "logical counter-proof," that the inheritance of acquired characteristics is "inconceivable."

CHAPTER XIV

THE IMPOTENCY OF SELECTION

How would it be if we should turn the tables on the Weismannites?

In the big hall of the British Museum in London, near the entrance there is a glass case containing stuffed cocks with tail-feathers many feet long. These are specimens of a Japanese race of the Phœnix Tosa or Yokohama poultry (Fig. 12) which in textbooks (Romanes, Weismann, Hesse-Doflein) are paraded as striking evidence of creative selection. On the glass case of the British Museum, a sign reads: "Long-tailed breed. A remarkable variation from the ordinary condition, produced by artificial selection." There are similar labels attached to specimens in the American Museum of Natural History and in the Bronx Zoölogical Park in New York. It was a researching scientist of London, J. F. Cunningham, who, as far back as 1903, exposed the fairy tale of the selective origin of the monstrously long tail-feathers.

Generally, one had been unable to renounce the impression that the Japanese always select those roosters which have comparatively the longest tail-feathers to intensify this characteristic to an extreme degree. This procedure is nothing but a myth. According to Cunningham's and the later reports of Davenport (1906), the tail-feathers of a cock which are intended

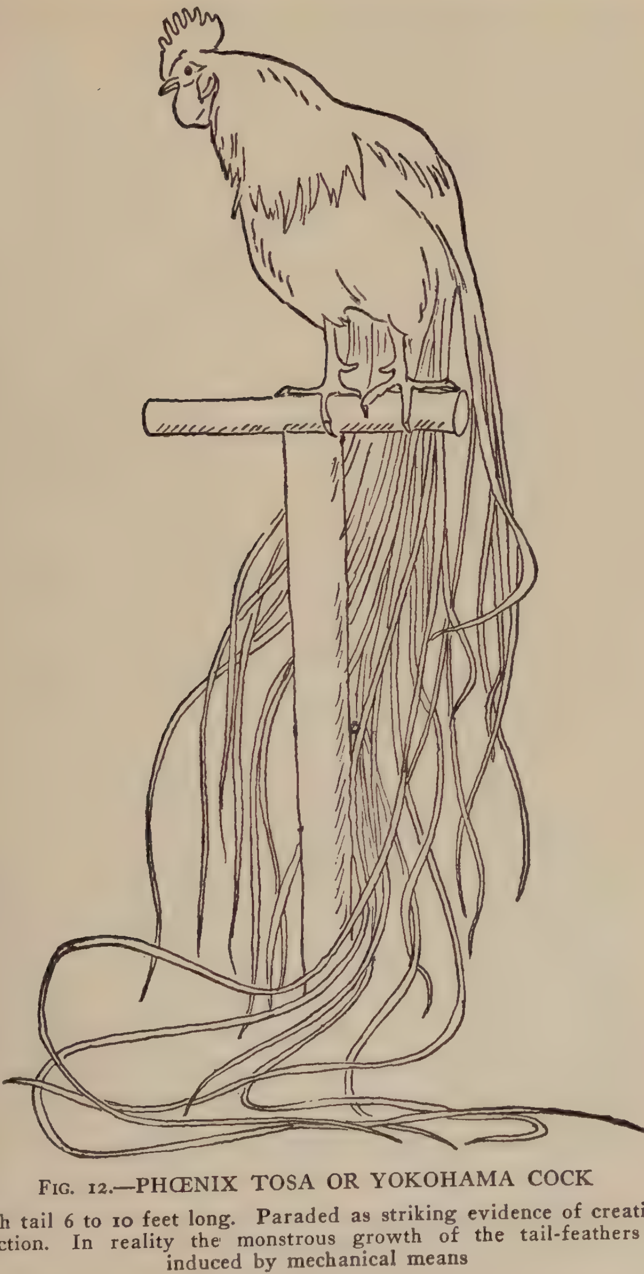


FIG. 12.—PHŒNIX TOSA OR YOKOHAMA COCK

With tail 6 to 10 feet long. Paraded as striking evidence of creative selection. In reality the monstrous growth of the tail-feathers is induced by mechanical means

to grow extremely long are massaged and wound on a spool. In both cases, a tension is created which extends to the feather follicle, *i.e.*, the little pocket in the skin from which the feather is growing. The mechanical tension results in a stimulation of growth —the feather growing longer than without tension. As soon as the feather ceases to grow, it is plucked and the succeeding feather will grow even longer, because of the stimulated feather follicle.

Aside from this data reported in scientific literature, I am in possession of some private information along the same line, for which I am indebted to Inspector Emeritus Kraus of the Menagerie at Schönbrunn. Kraus, who accompanied the Archduke, Franz Ferdinand d'Este, on his trip around the world, found occasion to watch the Japanese "manufacture" long-tailed cocks. A young specimen was posted on a perch in a very narrow bamboo cage, many feet high. A cage of this kind was brought back from Japan. The sickle-shaped tail-feathers were then held firmly down with little weights. These weights pull on the quill and vane of the feather and this stimulates the feather follicle into an intensified growth. This method, as one may observe, varies slightly from the one described by Cunningham and Davenport, but the underlying principle is identical. The weighted feather grows abnormally long and to prevent the cock from breaking it, the cage must be very narrow so that it is impossible for the cock to move around much. The height of the cage is necessary so that, with the gradual lengthening of the tail-feathers, the cock may be posted on a higher perch as soon as the perpendicularly hanging down feathers strike the floor.

Phœnix cocks that are not subjected to such a treatment grow tail-feathers longer than the ordinary domesticated cocks; but they are never as long as six to nine feet, as in cases of cocks that had had their feather follicles stimulated. Now, do the Japanese breed this race because the cocks, from the very beginning, had tail-feathers somewhat longer than usual and, therefore, this inborn characteristic affords a possibility of lengthening still more the tail-feathers by mechanical influence? Or is it possible that the often-performed artificial lengthening of the tail-feathers has already become a characteristic of the race to such an extent that a certain lengthening of the feathers would come about without artificial stimulation? I assume the latter viewpoint, but as yet nothing has been established with any certainty in this respect. Would it not have been much more advisable and of greater scientific value to employ ordinary domesticated cocks in research purposes to accomplish more dependable results, instead of reanimating time and again the fairy tale of selection, in spite of all explanations to the contrary?

Until recently similar myths accompanied other results of refined breeding of the old established and celebrated animal and plant raising of the Far East, surrounding them with its mysterious glamour. It is almost common knowledge nowadays that dwarf trees, in this country usually referred to as "Japanese gardens," are not results of a regular selective method, *i.e.,* that they have been produced by a method extending over several generations. We know now that these dwarfed specimens are results of maltreatment, under-

nourishment, insufficient room for their roots to spread, and general mutilations.

An analogous story may be told regarding the monstrous races of goldfish of Japan and China, which reached the aquariums of the Western world and now propagate themselves hereditarily true to form. Among these fish are the Fringe-tail (*Carassius auratus var. japonicus bicaudatus*) and the Comet-tail (*C. a. var. japonicus simplex*) with their superfluous, powerlessly drooping, overgrown fins, the Telescope (*C. a. var. macrophthalmus*) and the Celestial Telescope (*C. a. var. uranoscopus*) with monstrously protruding, twisted, blind, or half-blind eyes; the Lion-head with intestine-like protuberances; the Fan-tail (*C. a. var. oviformis*) with its inflated, balloon-shaped body. All these deformities which were supposed to be the result of odd and super-refined breeding experiments (according to observations by v. Kreyenberg in China, experiments by Tornier, 1908, and investigations by Milewski) now turn out to be utterly unintentional products of degeneration. The Chinese and Japanese merchants keep the goldfish in indescribably neglected, dirty, clay vases which are put one on top of the other and are, therefore, mostly dark and badly ventilated. In such "prisons," among normally proportionated fish, quite often specimens develop which "all by themselves" show the aforementioned deformities. When "the most beautiful" of them were isolated, it became evident that they passed on their deformities true to form and hereditarily.

In all probability, the so-called Japanese "dancing-mice" (Fig. 32) are not bred intentionally, but are the products of either extreme neglect or cruelty. This

becomes evident from the unnatural character which is their main peculiarity—an insanely fast twirling. I noticed a similar irresistible impulse in a specimen of the European "wood mouse" (*Mus sylvaticus*). After the rodent had not been fed for two days it twirled round. The habit of twirling madly remained long after the mouse had been fed sufficiently and regularly. It was not, however, possible for me to observe whether the twirling was hereditary in this case. But as far as it is permissible to draw deductions from a single instance, I am under the impression that the development of such an irresistibly forced motion of a wild-living species of mice is quite remarkable and worthy of study. I made exactly the same observation on a "house mouse" (*Mus musculus*); but here it could be claimed that the specimen was a hybrid of an ordinary mouse and a so-called dancing mouse, a possibility which in the case of the "wood mouse" seems entirely excluded. For this reason, the twirling habit of the latter could not have been inherent, but was simply "released" by hunger.

Griffith, and after him Detlefsen, fastened cages (each one of them smaller than a foot in diameter) on a circular board which was set in motion and rotated horizontally sixty to ninety times a minute, uninterruptedly, day and night. Rats kept in these rotating cages for the duration of several months, when released always moved in such a manner as if they had to counteract a rotating movement. That is, that now released from the rotating cages, they run around in circles as before. Traces of this acquired trait were evident even in the offspring of the second generation. Here we have a counterpart to the Japanese dancing

mice, in witnessing the development of the "American dancing rats."

It is claimed by Detlefsen that due to the breaking down of one static organ in the ear of the rats, certain bacteria infected this ear. The bacterial infection caused, in turn, the locomotive disturbances in the first generation and—by directly infecting the offspring—in subsequent generations. Animals with infected ear labyrinths showed the locomotive disturbances also in motionless cages, independently whether they descended from parents which had lived in rotating cages or not. Accordingly, we may conclude that this is not a case of inheritance, but merely the result of an infection in the adults and young, living in a common, infected abode.

The most conspicuous and scholastic examples of productive selection (with which can be proven that the much-talked-about selection did not play a part at all in these instances) are supplemented by selection experiments which lead up to the conviction that selection alone can never be productive. Far from being "omnipotent," as Weismann's disciples claim, selection is even impotent, as far as productive results are concerned, because it is impossible for selection to either induce new characteristics or intensify established ones beyond the limit of the tendency inherent to the race concerned. The appearance of unknown characteristics as a result of exterior influences seems neither improbable nor impossible to conceive; but it *is* improbable, and it is also impossible to conceive the accomplishment of the same result as an apparent effect of selective processes, and as a result of determinants which have lain hidden in the germ plasm.

As selection is not within the intrinsic scope of this book, we are not going to prove this assertion in the present volume. The proof, however, that natural as well as artificial selection plays only a negative (*i.e.*, a sifting) and a conserving (*i.e.*, mantaining and apportioning) part with all the exactness to be desired was offered first by W. Johannsen, with his basic bean experiments which most probably will survive from his theory of pure strains (*biotypes*).

Thus the objection raised against the inheritance of acquired characteristics, namely: that we are dealing here with selection only, is completely disposed of. This objection may be considered in the future as being eliminated, because where something is produced (aye, where something is only changed!) it cannot have been effected by selection. Selection itself is not productive! Even if the assumption of the opponents were right—that the experimenter very easily could carry on an unintentional selection favoring his ends—the strength of evidence of an experiment affirming the inheritance of acquired characteristics from now on could not be minimized any more.

Selection could only tend to speed up, and in this way stimulate new developments, which in any case are brought about by exterior influences. It is an exterior influence which blackens the wings of the butterflies and lengthens the tail-feathers of poultry. If at this very moment selective measures are taken when the process of changing is continued, when the most distinct cases of dimming the wing colors and lengthening the tail-feathers are selected, success is naturally assured. But this is only apparently a positive result of selection. In this case, selection only sup-

ports an agent derived from exterior influences already present which, even without selection, would have effected changes, albeit more slowly.

In all probability, a similar reason is underlying all those cases where the deciding factors have not been discerned as yet, due to the prevailing blindness to environmental influences. This is evident in cases where the breeder comes to the conclusion that, by painstaking selection of most favorable breeding results, he is effecting what in reality is effected by environmental factors (condition of the stable, food, etc.) which, although unknown, continue to affect the breeding one generation after the other, and thanks to the fact that the most sensitive specimens are always chosen, there finally is a general intensification of the breeding results.

If, for example, cattle are bred which give large quantities of milk, the success of this breeding is based not only on the fact that the best cows and the best bulls have been chosen for breeding purposes, but also upon the fact that the cows and their forebears have been thoroughly tested and subjected to intense milking (Chapter XLVI). From where else should one know that here one is dealing with cows which may be depended on to give large quantities of milk? In this case, selection is necessary only to prevent a depreciation of the high standard achieved, which could result from false methods of crossing, for example, by choosing a bull of a milk-poor breed.

In cases of breeding race horses, one should not overlook the fact that the success of breeding, generation for generation, is controlled by testing the speed, and by training the mount.

The same holds true in breeding very fertile species of poultry. Rapid laying of eggs in itself is ever and again responsible for an additional super-production of eggs. How is it possible to underrate, and even to deny, all these factors which play a decided part in the development of a race and which under no circumstances can be eliminated?

CHAPTER XV

EXPERIMENTS ON SALAMANDERS: REPRODUCTION

AFTER having reviewed critic and anti-critic, we return to the description of additional breeding experiments which were instigated to test the inheritance of acquired characteristics. But while the previously related experiments on butterflies and beetles, were helplessly delivered up to objections as enumerated before, the following experiments were conducted with a view to effectually avoid as many objections as possible.

As far as my own research work is concerned, aside from the Midwife Toad, the spotted or Fire Salamander (*Salamandra maculosa*; Figs. 13 to 20, 22, 23) with an habitat in the moist woods of Europe, has become my favorite (Kammerer, 1907 a-c).

The spotted salamander bears about fifty young, which, for months after birth, do not resemble the mother but live in water as larvæ, with clusters of gills for respiration and with a finned tail for swimming. If one removes the female from the water, precluding the possibility of giving birth to its young in a moist element, it has, of course, hardly any perceptible effect upon the development of the next pregnancy. Floundering larvæ are born on dry ground, and they would invariably perish if they were not promptly placed in water. Death by drying up would

Fig. 13

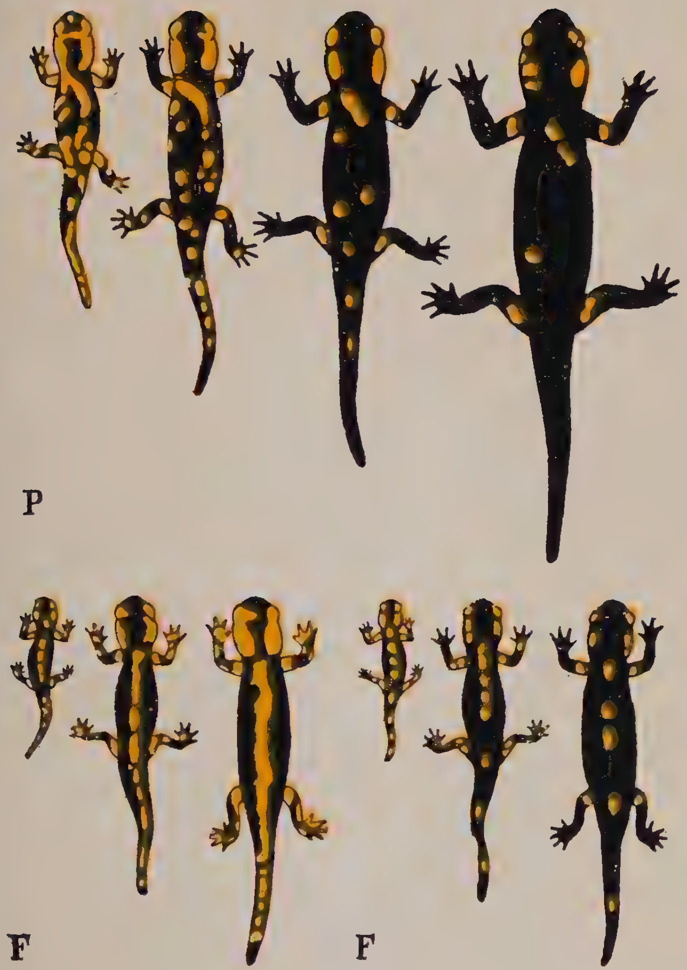


Fig. 14

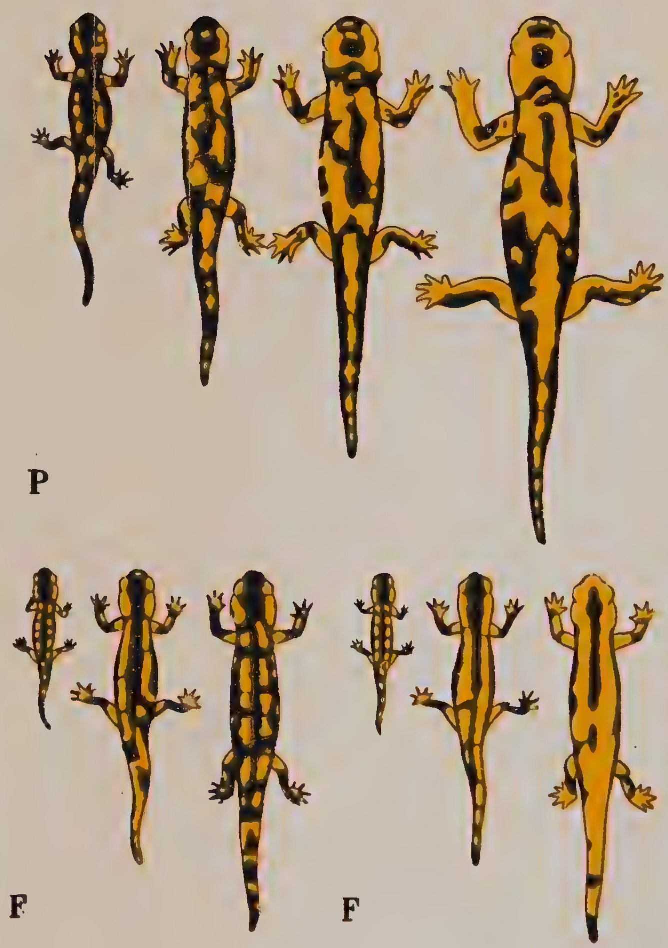


FIG. 13.—SPOTTED SALAMANDER (*Salamandra maculosa*)

Color changes when kept on a *black* background. P, Development of the mother in stages two years apart (*i. e.*, six years on a *black* background when at the last stage shown here). *F*, Development of offsprings of the former when kept on a *yellow* (left) or on a *black* (right) background, in stages one year apart

(Kammerer, semi-schematic)

FIG. 14.—SPOTTED SALAMANDER (*Salamandra maculosa*)

Color changes when kept on a *yellow* background. *P*, Development of the mother in stages two years apart (*i. e.*, six years on a *yellow* background when at the last stage shown here). *F*, Development of offsprings of the former when kept on a *black* (left) or on a *yellow* (right) background in stages one year apart

(Kammerer, semi-schematic)

also have been the fate of the next issue—usually born at intervals of six months—had not the mother salamander delivered larger larvæ which, within the womb of the mother, passed the period meant for development in the water. Generally, beginning with the fourth pregnancy, at the conclusion of the second year of experimentation, the young ones, born on land, are no longer in any danger of death by drying up. They are completely developed little salamanders breathing through lungs and, thanks to sturdy little legs and a cylindrical finless tail, they have the ability to move with ease upon solid ground.

The successfully acquired advantage of depending on the mother during the entire period of development instead of on the water, certainly had to be compensated for in some manner; so instead of fifty progeny (the usual potentiality of the female salamander), only six, four, or even two are born at one time, the salamander's womb allowing no space for more. The remaining eggs liquefy at an early stage and develop a fluid yolk, which serves as nourishment for those embryos destined for further development.

After the necessary space and nourishment within the narrow storehouse is provided for, there remains only the respiration to be looked after. This means that the salamander must do the best it can to get along on the scantiest supply of air possible. As an increase of air through ventilation is impossible, the amount available must be utilized to the fullest extent by the respiratory apparatus. This apparatus—the gills—enlarges its respiratory surface, becomes elongated, branches out, increases the number of blood

vessels, and covers itself with a very fine skin in order to allow no hindrance to the entrance of air.

The newly born salamanders display still another change; aside from being subnormal in size, they bear noticeably few yellow spots and, in fact, are almost monotonously black. Pogonowska (Lemberg) has proved that this same result appears if the salamander larva is cultivated in water containing common salt. This experiment also explains why the larvæ turn black in the womb; namely, because animal tissue is also salt-containing, and this is particularly true of the fluid-filled cavities, such as the pregnant uterus. Larvæ, remaining in the womb, live therefore in a salt-containing medium.

The insignificant trick of the experimenter, *i.e.*, the preventing of the mother salamander from disposing of its young in the water, brought about not only one, but an entire series of different characteristics. Since these made their appearance during the individual life of each salamander, as a result of external events, and since they also indicate the personal experiences of each generation, one may designate the late birth, the diminished number and size of the progeny, the more pronounced development of the gills, and the fainter spotting, as "acquired" characteristics.

With sufficiently frequent repetitions of similar experiences late-births become not only a fixed habit of the mother (so that even in sufficient water she develops a reduced number of young within the womb) but the young also become late-bearing. Subsequently the entire chain of development (size, gills, coloring) apparently is inherited by following generations and has become general.

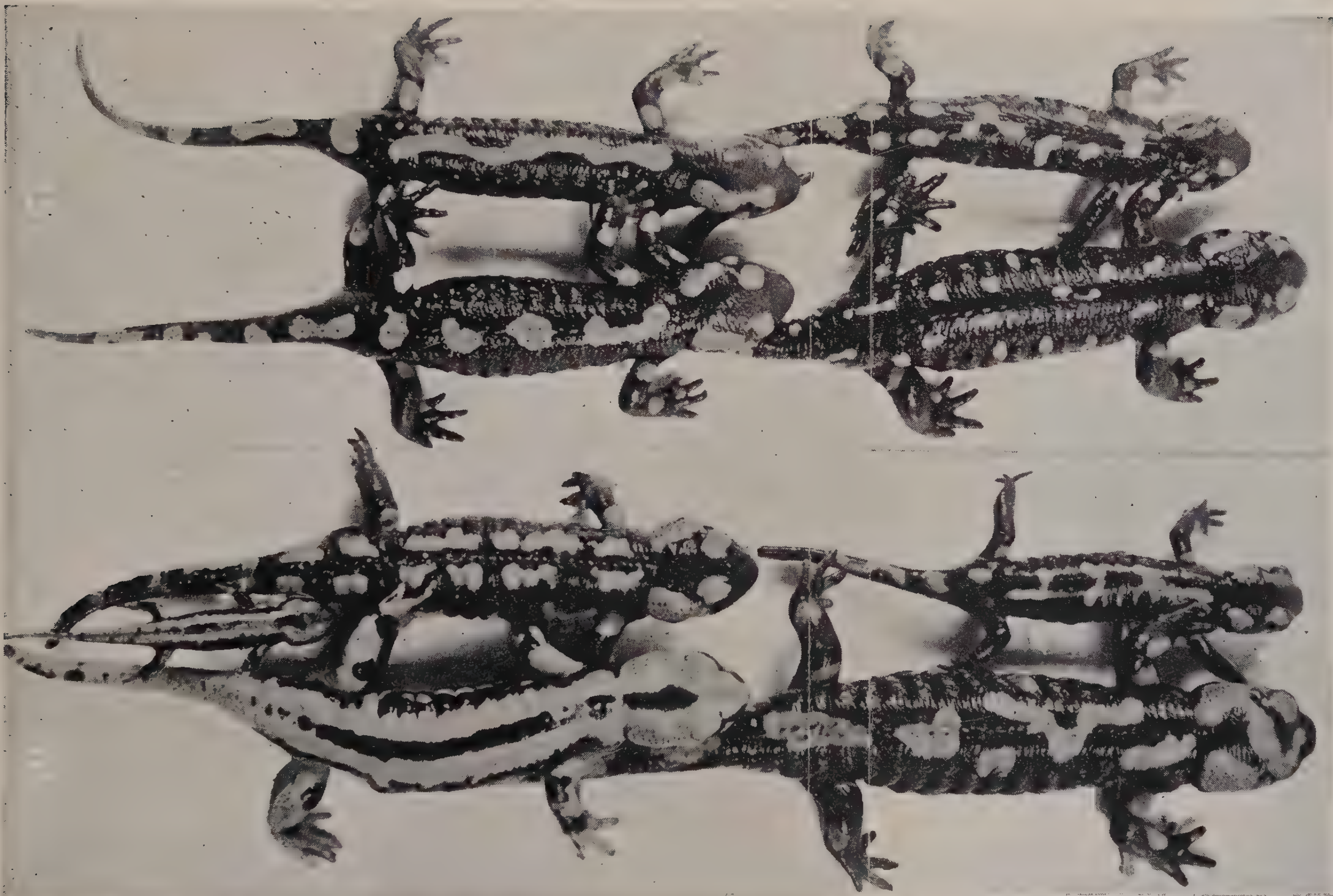


FIG. 15.—SPOTTED SALAMANDER (*Salamandra maculosa*)
(Color changes when kept on a *black* background)

Upper row: Parent generation. Lower row: Children generation. Upper row, left: Uninfluenced, partly mature specimen, as were used for experimental purposes. Upper row, right: Final stage of color change in the first generation. Lower row, left: Specimen of the second generation, raised on a *yellow* background. Lower row, right: Specimen of the second generation, raised on a *black* background as the parents were

(Original photo on the same negative, Stewart, Cambridge)

FIG. 16.—SPOTTED SALAMANDER (*Salamandra maculosa*)
(Color changes when kept on a *yellow* background)

Upper row: Parent generation. Lower row: Children generation, and one grandchild. Upper row, left: Uninfluenced, partly matured specimen, as were used for experimental purposes. Upper row, right: Final stage of color change in the first generation. Lower row, left: Specimen of the second generation, raised on a *black* background. Lower row, right: Specimen of the second generation, raised on a *yellow* background, as the parents were. Lower row, center: Specimen of the third generation, just metamorphosed offspring of parents which for two generations were kept on a *yellow* background

(Original photo on the same negative, Stewart, Cambridge)

This entire experiment has already been performed for us by nature, which has brought to existence a type of salamander inhabiting the mountains where bodies of water, favorable to depositing the larvæ, are lacking. The black salamander of the Alps (*Salamandra atra*) is apparently a hereditarily degenerated descendant of the spotted salamander or a mutual forebear of the two species. It gives birth to two fully developed young which pass their larval state in the maternal uterus, maintaining life by consuming the other eggs and by the development of large gills.

CHAPTER XVI

EXPERIMENTS ON SALAMANDERS: COLOR

The spotted or Fire Salamander is ink black and more or less marked with yellow. In the Vienna beach woods, where I collected the greater part of my experimental material, the yellow marking consists of irregularly distributed yellow spots.

My experiments (Kammerer, 1910 c, e; 1912 a; 1913 c) started with this material (*forma typica*). They are kept in a colored surrounding and, as far as the main experiments are concerned, in an environment which showed the same colors as the experimental specimens, *i.e.*, yellow and black. Whether, in order to create environment with these colors, we choose yellow clay and black dirt, or whether we use colored paper under the bottom of an empty glass receptacle in which the salamanders are kept, or whether we finally paint the aquariums yellow or black, respectively—as was done by C. Herbst and E. G. Boulenger, who repeated my experiments—makes, to be sure, a slight difference in the details (which I do not intend to mention here) but in the main amounts to the same.

If spotted salamanders are kept on a black background (Figs. 13, 15, and 17), the yellow markings are reduced for the benefit of the dominant black color. After a few years, these salamanders appear mostly

lack. Their offspring, again kept on a black back-round (Figs. 13, 15, right-hand corner, bottom row), 1ow, especially along the middle line of their backs, single row of small circular spots. On offspring hich, contrary to their parents, were raised on a yel-ow background, these spots merge into a band (Figs. 3, 15, left-hand corner, bottom row).

If, in contrast to the previously related experiment, potted salamanders are kept on a yellow background Figs. 14, 16, 18), the yellow markings enlarge at the xpense of the dominant black. After a few years, iese animals are mostly yellow. If half of their off-pring are raised in yellow surrounding (Figs. 14, 16, ght-hand corner, bottom row), the yellow color preads even more and appears in broad, regular, bi-teral stripes along both sides of the body. The other alf of the offspring, raised on a black background, de-elop less yellow but quite an amount, compared to the olor of the environment which stimulates a contrasting oloring. In this case, also, the salamanders show egular, bilateral rows of spots along both sides of the ody (Figs. 14, 16, bottom row left).

What utterly surprised me was the fact that, in the econd generation, the asymmetrical marking changed a symmetrical marking in the form of spots or tripes. The two groups of experiments supplement ach other to such an extent that it is most remarkable witness the accuracy with which the living substance eacts. Aside from certain spots to be observed in airs on every salamander—on the lids of the eyes, on ie ear-glands (*Parotides*) and at the sockets of their mbs—the photographic negative of the one row of xperiments looks almost exactly like the positive of

the other row. To elucidate: If, on the offspring of the one group of experiments all yellow spots were painted black, or vice versa, one group would be almost identical with the other.

Both have it in common that the dominant color—black in the one case, yellow in the other—spreads along the sides of the body, while the other color recedes to the center of the body. Specimens with a yellow center line, as brought about by this experiment, are very rarely to be found in the woods; but I could breed any number of them in the laboratory, being absolutely sure of success. Specimens with two yellow stripes along their sides have never been found in the neighborhood of Vienna. But in North Germany and in other regions especially where yellow clay (or clay slightly reddish on account of oxide of iron) forms their environment, they developed into a special race (*Var. tæniata*).

In the neighborhood of Heidelberg (and probably in other places) it is interesting to notice that the symmetrically striped race even today still testifies to its originating from the asymmetrically spotted form. This is proven by the breeding experiments conducted by C. Herbst with the material from the neighborhood of Heidelberg, who involuntarily, in this way, corroborated my color changes. The salamanders of the neighborhood of Heidelberg, freshly emerged from the larvæ, are asymmetrically spotted and only during their period of growth arrange their markings into the bilateral, symmetrical design. In still other districts (for example, the Hartz Mountains, Germany), the young, immediately after the metamorphosis, *i.e.*, right after assuming their terrestrial form, display the

FIG. 18.—SPOTTED SALAMANDER (*Salamandra maculosa*)

Change of color on specimen kept on a *yellow* background. *a*, dorsal view; *b*, ventral view; *c*, right side. The specimen was the offspring of irregularly spotted parents which, kept on a yellow background, had become richly yellow

(Kammerer)

striped marking which at times needs only slight corrections to be absolutely regular in appearance.

The development of the spotted form into the striped one is reversible. It is also possible to change striped salamanders into irregularly spotted specimens. This has been corroborated by E. G. Boulenger, who kept a number of his experimental specimens, during their larval state, on a colored background. In this way he achieved results which are more beautiful and more striking than my own.

CHAPTER XVII

CROSSING AND GRAFTING EXPERIMENTS

At the end of the experiment to change the colors, I have two "breeds" of bilaterally striped salamanders at my disposal: first, those produced by Nature, which I imported from the Hartz Mountains; second, those specimens which were bred from asymmetrically spotted parents in the laboratory. For this reason, I had an old established natural race and a new laboratory race, the two being exteriorly identical to each other. I used both for the purpose of crossing and transplantation experiments (Kammerer, 1910 f; 1911 a, b; 1913 c).

If spotted salamanders be crossed with naturally striped salamanders (Fig. 19), the progeny are subject to the Mendelian Rules. First, to the rule of alternative inheritance: all members of the first crossing generation (all progeny of the crossing) are asymmetrically spotted—spottedness is dominant over stripedness. They are, secondly, subject to the rule of segregation: seventy-five per cent of the second crossing generation (the grandchildren) are again irregularly spotted, while the remaining twenty-five per cent diverge from the purely original race of the grandparents.

Just one word more before proceeding: does not the dissociation of the grandparental original race, as

Fig. 19

Fig. 20

FIG. 19.—SPOTTED SALAMANDER (*Salamandra maculosa*)

Crossing the asymmetrically spotted race (*forma typica*) with the symmetrically striped race (*var. tæniata*) as found at large. It is irrelevant which race is drawn upon for the father or the mother

FIG. 20.—SPOTTED SALAMANDER (*Salamandra maculosa*)

Crossing the asymmetrically spotted race (*forma typica*) with the symmetrically striped experimental race (*var. tæniata*). *P*, parents; F_1, children; F_2, grandchildren

brought about by hybridization according to Mendel's method, prove the genuine inheritance of acquired characteristics ("somatic induction," Chapter IV) to be unthinkable? If we cross a black animal with a white one (Fig. 21), and all children are black, propagating

FIG. 21.—SIMPLIFIED SCHEMATIC CHART OF MENDELIAN RULES
(Rules of Dominance and Segregation)

The large circles indicate individuals. The small ovals within these circles denote the germ plasm of the individuals. The "white" germ plasm remains uninfluenced in black individuals
Top: parents. Center: children. Bottom: grandchildren
(Original)

among themselves, only one out of every four grandchildren will be white and three black. For this reason, the exteriorly, entirely black children, aside from a tendency (determinant) towards black, must have had a determinant for white which they inherited from

one of their grandparents. Moreover, the germ cells with a determinant for white must have remained completely uninfluenced and of an entirely pure strain in the black body; while the latter does not impart its color to germ cells differently disposed. Apparently, the independency between body and germ plasm, expounded by Weismannism, is here glaringly verified by Mendelism.

Returning to our salamander crossings, however, breeding experiments take an entirely different course if we use, instead of the naturally striped salamander, a specimen of the same marking raised in the laboratory, and cross it with a spotted salamander (Fig. 20). The hybrids (the children) in this case are also spotted—not asymmetrically, as in the first crossing experiment, but striped-spotted. For this reason they rank between the spotted and the striped parents. Propagating among themselves, the second crossing generation (the grandchildren) are all marked with stripes, broken up into spots. The Mendelian segregation of continually striped specimens cannot be observed.

This crossing teaches us a difference between new and old characteristics. If, by chance, they are exteriorly similar to each other, new and old characteristics become distinct on account of the way they react in crossing experiments. Doubtless, the two are hereditary, but only the old race character, as far as the salamander is concerned, obeys the Mendelian Rule; while the new character defies this rule, showing no atavism to the markings of the grandparental races. After taking cognizance of the transplantation experiments, the theoretical importance of this fact

will become even more striking and more interesting if one considers that the overwhelming majority of Mendelian experiences was observed only on very old race characters.

At any rate, some cases are related where the "acquired characteristics" immediately act according to the Mendelian Rules. Within this scope belong the experimental types of the Colorado Potato Beetle (Fig. 5), as bred by Tower, and which, when crossed, did not distinguish themselves from those of similar, natural types. Another case is that of a crossing of the normal Midwife Toad (Chapter XI) with specimens which deposit their eggs in the water and do not take care of them any more. To be sure, a certain complication arises here, inasmuch as the dominant characteristic (*i.e.*, the one preponderantly in all children and three-quarters of the grandchildren) follows the father and, for this reason, a change of dominance from the normal to the changed form is to be observed, depending on whether a normal specimen or a changed specimen plays the part of the father. In other cases, it is of no importance which race in crossing experiments is employed on the part of the father, and to which the part of the mother is apportioned.

A very remarkable experiment of Hoge should be related here: He ascertained that *Drosophila* (a small fly to be seen swarming around decomposing fruit) when bred in low temperature was inclined to grow additional legs. Probably the low temperature affects certain groups of cells which break up and, in this way, separate sockets for growth of limbs are produced. This disposition to a multiplication of limbs is not only passed on by inheritance but even obeys the Mendelian

Rule if specimens bred in low temperature are crossed with normal ones under normal temperatural conditions.

In the previously related case of the Midwife Toad (Chapters X and XI), the conformity of this experiment with the Mendelian Rule, as to crossings, is easily understood because, strictly speaking, there were no "new" characteristics, but only a resurrection of an old characteristic which probably, for the time in between (though not in evidence), was still retained as determinant within the germ plasm. In the other enumerated cases of conformity to the Mendelian Rule, the assumption to deal with the atavism is less evident and, especially in the case of *Drosophila,* seems entirely unsubstantiated.

For this reason, we will have to deal with the possibility that, occasionally, genuine new characteristics immediately become subject to the Mendelian Rule. As far as the color of the salamander is concerned, this up to now did not hold true. For this reason, for the time being, we are limited to the salamander experiments to differentiate between old and new characteristics. This quite practical possibility of differentiation in crossing experiments is perfected by the way the ovaries of salamanders reacted when transplanted.

If ovaries of spotted females are transplanted into those that are striped by Nature (Fig. 22), the young will always be marked according to the origin of the ovaries. They are always irregularly spotted.

If, however, ovaries of spotted females are transplanted into artificially striped ones whose parents were still spotted (Fig. 23), the offspring of a spotted

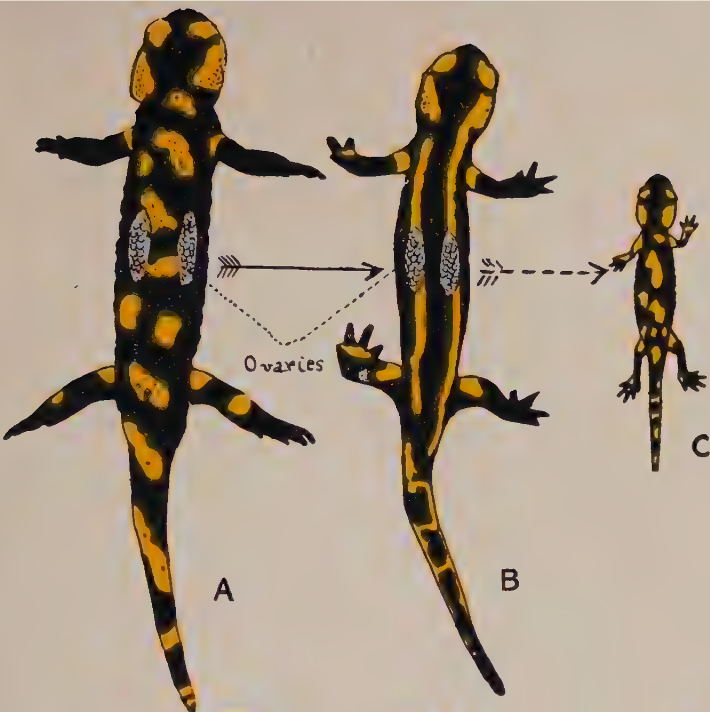


FIG. 22.—SPOTTED SALAMANDER (*Salamandra maculosa*)

splantation of the ovaries of an asymmetrically spotted specimen (*forma typica, A*) into a
etrically striped specimen of the "natural" race (*var. tæniata, B*). The progeny (*C*),
according to the true mother, is in any case irregularly spotted.—(Original)

FIG. 23.—SPOTTED SALAMANDER (*Salamandra maculosa*)

splantation of the ovaries of an asymmetrically spotted specimen (*forma typica, D*) into a
etrically striped specimen of the "artificial" race (*E*). The progeny, according to the
-mother, is spotted-striped (*F*), in case the father was spotted; and wholly striped (*G*)
if the father was striped.—(Original)

father are stripe-spotted; the offspring of a striped father are uninterruptedly striped.

From all this, it becomes clear that the ovary of the spotted female brings into the body of the naturally striped foster-mother only its own hereditary properties. Deciding for the marking of the progeny is exclusively the origin of the ovaries. The marking of the body in which the progeny develops has no influence whatsoever on the young.

This result is in accord with all transplantation experiments in which offspring were produced from transplanted generative glands, provided that the transplantation was performed with the greatest accuracy. Only lately, this has been proven by the results achieved by B. Wiesner, who exchanged the ovaries of black and white rats. Of course, it is to be taken into consideration that, in performing these ovarian transplantations, attention was always exclusively paid to old established race characteristics and not to characteristics acquired by the body of the foster-mother, or those acquired by her offspring.

In the latter case, the result would be an entirely different one; for, in the body of an artificially striped foster-mother, this same ovary of the spotted female behaves exactly as if it had been derived from the body of a striped female and just as if the eggs of the striped female had been used in the crossing (Fig. 23).

The objection cannot be raised that the operation was not thorough—that portions of the original ovaries may have been left behind in the foster-mother, as in Guthrie's experiments on fowls, which were afterwards tested by Davenport (1911) and found to be possibly merely cases of regeneration of the original

ovaries. Thanks to its wealth on connective tissue the ovary of the salamander can easily be removed from the surrounding tissue as a whole. It is impossible that any remnants could have been left behind and that the descendants were derived from these remnants regenerated.

Let us now try to explain the crossings and ovarian transplantations observed in the salamander on the basis of one and the same reason. Cunningham (in *Nature,* 1923, p. 702) states that to him a contradiction, an obstacle for a common explanation, arises from the fact that, in the crossings, the naturally striped specimens (but in the ovarian transplantations the artificially striped race) brought about more effective hereditary results. MacBride (in *Nature* of June, 1923, p. 841) rightly opposes Cunningham's conception. In the crossings, as well as in the transplantation experiments, it is the newly evolved striped race which develops the more powerful hereditary potency. Of course, one should be careful not to fall under the spell of the prejudices as if the Mendelian Rules constitute the highest development of hereditability. If one succeeds in preserving one's mental independence from Mendel's scheme, one will notice that, as far as the new striped race is concerned, the whole progeny —the children and also all the grandchildren—inherit the striped design, while regarding the old striped race, certain percentages of the progeny—all children and three-quarters of the grandchildren—exteriorly remain uninfluenced by the tendency for stripedness. This is in entire conformity with the observation that this characteristic, when developed on the body of a

naturally striped female, does not result in color changes at all in reference to the eggs of a spotted female, while it results in extensive color changes in cases where an artificially striped female has been employed as foster-mother.

CHAPTER XVIII

MENDELISM AND LAMARCKISM RECONCILED

THESE quite regular differentiations of old and new, inherited and acquired characters (stripedness of the salamanders) in transplantation and crossings, I have tried to make intelligible by an analogy which I confess is rather provisional and crude. A new piece of clothing or jewelry, for example, irritates. So does a pair of new shoes or a new ring. This irritation consists of a slight pressure which diminishes the longer the clothing or the jewelry is worn, and ultimately disappears. In time, one simply becomes used to this irritation.

Analogous to these mechanical irritations are morphogenetic ones, *i.e.*, such irritations as originate from the mere presence of each part of the body. These morphogenetic irritations are capable of wearing off and usually wear off. The more recent the changes, the stronger the irritation, and in all probability the more different as to quality. Under favorable conditions of duration and intensity, the irritation penetrates to the germ plasm. There it renders permanent a potentiality for repeated actual change in the form of a determinant.

The details of the process of passing on acquired characteristics have been exhaustively described by Roux. Weismann has spoken of them as "unimagi-

nable" (*"unvorstellbar"*). But surely it is much easier to imagine them than Weismann's own highly intricate and involved construction of the germ plasm, which assumes as a foregone conclusion—to express it very popularly—that all the generations of man have already lain fully prepared in Eve's ovary. The passing on and absorption of an acquired characteristic, or the irritation released by it, may very well be compared with the telephone transmission of sound. The thinness and homogeneity of the wire does not represent an obstacle to the transmission of complicated sound waves, like those of a polyphonous orchestra or choir, which finally reach the human ear, to be preserved in the brain for later reproduction.

We said before that a newly acquired characteristic results in a morphogenetic irritation which, as an "irritation of the new," possesses a great radiating power and which, under favorable circumstances, penetrates to the germ plasm proper. The longer, however, a once "new" characteristic remains in the possession of its present owner, the more its morphogenetic irritation diminishes. This irritation no longer reaches the germ plasm, for the germ-plasmic transmission is already a thing of the past. It is now no longer necessary because the determinant belonging to it is now a part of the germ plasm, acquired at the time when the morphogenetic irritation originally set in. For this reason, there exists only a state of dependency between actually acquired new characteristics and the germ plasm. Between the germ plasm and old (possibly resurrected) characteristics there is that state of independency which is demanded by Weismann and proven by Mendel.

A confirming example of the foregoing has recently been published by W. Finkler. This example does not deal with an influencing of the germ plasm from the body, but deals with the influencing of two parts of the body. Most animals are more strongly colored on their dorsal side than on their ventral side. The contrary, however, may be observed on the boat-fly (*Notonecta*) which swims with its ventral side up, and in the common boat-fly (*Notonecta glauca*) the wing covers are almost uncolored. By illuminating from below the water in which they were kept, Finkler succeeded in engendering specimens with a marbled design like that of a related species (*Notonecta marmorea*) which lives in waters with a brightly colored and light reflecting bottom, and grows this marble design naturally.

Finkler now grafted foreign heads on the body of the boat-fly, as follows: (1) heads of the common boat-fly whose wing covers had been marbled as a result of having been exposed to light from below; (2) heads of the naturally marbled boat-flies. In the latter case, the head did not succeed in influencing the uncolored foster-body on which it was grafted. Only in the first case, the grafted head of an artificially marbled boat-fly succeeded in marbling the foster-body.

I only wish to add that in these grafting experiments, as in all normal controlling tests, the illumination came from above and not below.

With these experiments the contrast between Mendel's research work and the principle of the inheritance of acquired characteristics, "Lamarckism," seems satisfactorily reconciled. Let us once more return to the fictitious example which we resorted to in Chapter

XVII; the black body of the product of a crossing of black with white is not able to change the purely white germ plasm inherent to it in the direction of black, because in all those Mendelian experiments which came to a satisfactory solution (*i.e.,* with the exclusion of some unsatisfactorily solved cases) black was an old established, long ago determinantly fixed characteristic. If it would have had to be newly acquired, and if the determinant for black would have had to be injected into the germ plasm, the determinantly white germ plasm would hardly have remained spotlessly "white."

Following up the supposed process of induction, therefore, to its ultimate goal, one arrives at the assumption that very probably every influence derived from the outside results in changes, and that most likely every change radiates all around, and for this reason also penetrates the germ plasm. If only a very few changes are passed on to later generations, the reason seems to be that the changes that penetrated into the germ plasm were too weak and, at least as far as our senses are concerned, remain outside the sphere of observation. These changes may remain invisible for all time, but nevertheless one should not depreciate those supposedly invisible traces. In spite of being hidden, they may play one part or the other. If the influence which originated them is repeated or endures, in the course of time, and in the course of generations the effect may be summed up to a clearly visible result.

CHAPTER XIX

ENDURING EFFECT OR NUMBER OF GENERATIONS?

THE expression "in the course of time and in the course of generations," which closed the preceding chapter, makes it possible to make one more general suggestion, namely: to bring about certain changes on a living being, the number of generations is not as essential as the duration and strength of the influence.

First, Secerov (1914) and, then, C. Herbst, v. Frisch, Przibram-Dembowski, and, lately, E. G. Boulenger developed a process to bring about the color changes on the spotted salamanders in much shorter time than I had succeeded in achieving heretofore. Young spotted salamanders, as a rule (cf. Chapter XV), are not born as mature little specimens and surely not in the colors they show later on when fully developed. They are born as larvæ, living in the water, and breathing through gills. They are of a grayish brown, darkly clouded coloring, which shows very little of the later bright yellow and deep black.

Conducting my original experiments, I exposed the salamanders to colored environments only when their skin had already developed their respective colors, that is, after the metamorphosis of the larvæ into a full-fledged salamander, and after their definite colors had appeared. I simply distributed young, fully developed salamanders on black and yellow surroundings. But

the same method can be followed with the larvæ which, because they are younger, are much more pliable than ever so young, but already fully developed, little salamanders. The salamanders' larvæ react on the colors of the environment "secretly," so to speak, as this change of color develops unnoticeably. The definite coloring matter is not there yet, but if it is there it is not ready to manifest itself. The preliminary stages, however, in all quietness are prepared in such a way that the result is in conformity with the drastic color demands of the environment. Thus, the freshly metamorphosed land salamanders, developed in a yellow surrounding, are much more yellow; if developed in a black surrounding are much more black, than one could have expected from the coloring their parents showed.

I applied this process of accelerating the color changes to my own experiments, and thereby proved that it is possible to develop in one single generation salamanders as yellow as they are usually to be found only in the third generation (Kammerer, 1922). To make this clearer, I am now going to supplement my previously mentioned experiences with results I made by breeding salamanders by generations.

We will remember that (as related in Chapter XVI) salamanders which were kept for two generations on a yellow background developed a design with wide stripes along the sides of their bodies. The third generation is very yellow, even before it is subjected to a color changing influence. The stripes along the sides of the body are very often connected with crosswise bands (Fig. 15, bottom row). If those specimens, whose parents and grandparents were prepara-

torily subjected to a color change in the direction of yellow, are again put in a yellow surrounding, the yellow markings in the center of the back, already quite diminished, are entirely eliminated. We evolve salamanders which, on the dorsal side, are wholly canarian yellow.

A result just as far-reaching may also be effected by exposing the newly born salamander larvæ to a brilliant yellow background in such a way that very bright daylight (if possible, some rays of the sun) falls on the specimens (Fig. 24), provided the temperature of the water is not thereby raised too much. Frequently, the newly metamorphosed little salamanders show an almost spotless yellow all over their dorsal sides, and this yellow appears to be jagged along the edges, on account of the overlapping black of the ventral side. If the little salamander remains on the yellow background during the course of his development, sufficient time is given to him to develop a spotlessly yellow "complexion" by getting rid of all the black "impurities."

Just to show that we are dealing with a frequently observed regularity, the example of the salamanders may be again supplemented by a similar experience from the realm of the lower animals. One group of minute crustaceans, common in our fresh-water ponds, the *Daphnia* (Fig. 38, p. 175), annually undergoes cyclic changes which, as far as they refer to the genus *Moina*, can be drawn upon for corroborative purposes. According to Papanicolau, these changes are already noticeable on the eggs, and then extend equally to the eggs of the surviving mother and to those of her

FIG. 24.—TERRARIA
(Kept in a greenhouse)

Salamanders have to be exposed to broad daylight from above in order to effect the color changes
(Photo Dr. W. Klingelhöffer, Offenburg)

generations of offspring, which are born during the course of the year.

In spring, the eggs of the *Moina* are comparatively large, of violet color, and clearly transparent. With advancing season, they take on a more and more bluish color, become clouded and diminish in size. While during the warm months, one generation follows the other, the mother *Moina,* which first shed the shell that preserved it over the winter, remains alive and, so, becomes the grandmother, great-grandmother, great-great-grandmother, etc., of all the generations born during the warm season. All this time, the mother *Moina* continually deposits eggs, usually up to fifteen batches. And all the changes which successively become noticeable on the eggs of her "daughters" (*i.e.,* the diminishing in size and transparency and the discoloring from violet into blue) become also noticeable on the offspring from the eggs of the original mother *Moina.* From one generation to the other, as well as from one batch to the next, deposited by the same female the changes appear simultaneously. Thus, it becomes apparent that it is the *time* (in the case of the *Daphnia,* the season) which changes the generations, albeit not the number of the successive generations.

Another species of the same genus, the *Daphnia longispina,* living in the Lunzer Lake in Lower Austria, has a very low "helmet"; its abdominal sting is very short and is adjusted at a sharp angle. Fed well in a hothouse basin, Woltereck (1911 b) observed that the "helmet" gradually became higher, while the abdominal sting grew in length and straightened out. If natural environment is restored to this artificially

produced race before the end of two years, these changes will not have become hereditary, in spite of the fact that meanwhile about twenty generations have run through their life course. Later progeny, however, even under natural conditions, retain a noticeably high "helmet" and have a longer sting than the original specimens of the Lunzer Lake. This span of time, even on animals which produce generations more slowly than the aforementioned species, was sufficient to make a change enduring and hereditary.

CHAPTER XX

OBJECTIONS AND COUNTER-OBJECTIONS

LET us once more return to the color changing experiments on salamanders (as mentioned in Chapter XVI), from which we digressed only to draw upon an analogous example and to ventilate the problem theoretically. How about those ever and again reiterated objections (as enumerated in Chapters V and VI) which the experiments on salamanders were intended to evade? To what extent are these old objections or new objections still applicable?

In the first place, drawing upon the color changing experiments on salamanders, is it altogether permissible to speak about an inheritance of the acquired color changes, in spite of the fact that these color changes do not remain fixed in the progeny, but slowly subside if the offspring are not kept in a surrounding in accordance with the now prevalent color of their body? The grandchildren would be the latest generation apparently to return to that color scheme which had been their grandparents' original coloring. One could deduce from this that a diminishing of the color, which their parents had already acquired, amounts to a criterion of the non-inheritance of the color changes and that we are dealing here merely with after-effects and not with inheritance.

But here a number of things have to be taken into consideration:

1. Young salamanders kept in black surrounding, without their parents being prepared for yellow, turn still blacker in a much shorter time.

2. The progeny was not only restored to intermediate conditions (for instance a mixture of yellow and black backgrounds) as in most breeding experiments in regard to the inheritance of acquired characteristics. For example, in the experiments of Standfuss, Fischer and Schroeder on butterflies, and Sumner on mice who restored their experimental specimens into intermediate temperatures; but the progeny of salamanders were exposed to entirely contrary conditions. However, every effect must finally be disposed of by its own counter-effect. One would not stand on the ground of known physical laws if one would expect a different reaction from a living reagent. Deciding for the hereditary effect in our case is the tenacity with which the acquired color is retained, despite the contrary effect of the surrounding.

3. Even more than that: The yellow color of salamanders born by very yellow parents is even enhanced in the beginning, when exposed to a black surrounding by way of contrast. Without hereditary influence, this would not be possible. The spotted rows of newly metamorphosed specimens show a tendency to blend into stripes, just as in the case of salamanders kept on a yellow background. Only later on, these stripes again dissolve into spots.

In 1919, C. Herbst reproached me for not mentioning the intensification of the yellow color on specimens kept on a black background and *vice versa*. But

already in 1909 at the Congress of German Naturalists and Physicians at Salzburg, and again in 1910 at the International Zoölogical Congress in Graz, I exhibited my material (Figs. 13 and 14) in the form of wall-maps. And each time I directed attention to the intensification of a color change in contrasting surroundings which Herbst neither accepts nor recognizes as a manifestation of inheritance.

All this goes to show that the diminishing of acquired colors on a contrasting background is no obstacle to take inheritance for granted, provided the experiments are able to stand up against the other current of objections raised against them.

Well, now what about selection? It is there, but in an inverted sense. For experiments to be conducted on black background, these specimens were picked as were most richly spotted yellow; whereas for the experiments on the yellow background, those salamanders were chosen as showed only a small amount of yellow. I chose my material by negative selection (contrasting selection), for the simple reason that it would dispose of the objection that the specimens employed by me were especially suited to the respective color changes right from the beginning. Of course, avoiding the possibility of raising such an objection, I exposed myself to the probability that the so chosen specimens were especially unsuited. Those to be changed into black, as much as possible, often had a tendency for yellow, while those intended to produce a yellow race also encountered contrasting hereditary tendencies. That the blackest become the yellowest, and the yellowest the blackest, it was necessary to defy all the hereditary influences. And that is what really hap-

pened to such an extent that *all* the specimens (not as in the experiments of the butterflies, merely a part of them) were greatly changed, so that there was no occasion whatever for selection.

Now, is there not a possibility that in spite of all this, a part of the germ plasm in the black salamander may have a tendency toward yellow? Is it not possible that that which we newly imposed on the specimen (to which we educated the specimen, so to speak) had already lain hidden in it as a determinant? The salamander could be a hybrid in conformity to the schematic chart (Fig. 21) with a number of entirely different determinants in its body, which remained uninfluenced. By contrasting selection, this possibility is diminished to a certain degree; but it is only done away with entirely through the differentiation of old from new characteristics in crossings and transplantation experiments, as well as through the proof brought about by transplantation experiments that under certain conditions as mentioned before the influencing of the germ plasm from a differently colored body can become a fact.

But where is the atavism? Supposing that the forebears of the spotted salamanders of today were blacker, an intensification of their yellow color would amount to a new acquisition. But in case the forebears were more yellow, the gain on black would be a new acquisition. The same holds true here as in the case of the Midwife Toad where also (Chapter IX) only one of two variations could be atavistic. But in that case, the opponents could still insist that (now again drawing on the salamander example) not the characteristic to be more black or to be more yellow is passed on, but the disposition according to necessity

to become more black or more yellow. But this disposition could have been brought about by selection instead of direct influence of the color of the environment (of course not in the present generations where selection was excluded).

But even this additional objection is weakened by the transplantation of the ovaries. If it can be proved that a characteristic or, let us say, a disposition to develop this characteristic from the body by way of a stimulant, penetrates to the germ plasm, we do not have to worry any longer as to whether the respective characteristic is an atavism or has never really been there before. With the proof of the transmissibility from the body to the germ plasm, the possibility of the inheritance of acquired characteristics has already been established.

But the germ plasm is not influenced in this way from the body by an atavism. This is not necessary, as the determinant has long since become part of the germ plasm. Just this is the difference that became evident by crossing and transplantation experiments and which now also disposes of any additional objections. As selection neither creates anything positive nor intensifies a characteristic, and because selection does not offer the possibility of swinging from one side to the other, this objection is in any case thoroughly disposed of.

And finally: How about the direct influencing of the germ plasm? Secerov (1912 a) has measured the amount of light which is able to penetrate the interior of the body of the salamander. Only one-sixth of one per cent of the outer light reaches the ovaries and, being mostly neutralized by the different strata of the

body, is ineffectively colored. It is improbable, therefore, that there could have been any direct color influence on the germ cells and a color-adaptation by parallel induction.

The skin is especially well adapted to color changes, because the "Chromatophores," *i.e.*, the pigment cells responsible for the coloring of the skin, are imbedded here. It is these cells which, by contraction and expansion, by multiplying and diminishing, as well as by changing the color of their contents, decided the colored aspect of the specimen. Moreover, eyesight is an indispensable condition. Salamanders kept in the dark, or blinded, assume at most only a still darker color and do not show any further color changing.

Finally, strong illumination from above (Fig. 24) is necessary to facilitate the color changes. Even if the light is slightly diffused—as in a case of illumination from the side instead of from above—no color changes are effected.

If such sensitive organs, as highly specialized for the absorption of light as is the skin and the eye, need a great quantity of light to be influenced, how could the utterly unspecialized (*i.e.*, unsensitized germ plasm) be influenced with only one-sixth of one per cent of light penetrating to it? Semon (1912) already criticized the stressed logic of this assumption.

Just the same possibilities frequently, in spite of all probability, develop into facts. But one more thing which utterly does away with the supposition of a direct color influence on the germ plasm consists of the transplantation of the ovaries, which had previously proven to be very practical. The ovaries of the spotted specimen, by the result they produce after being

transplanted into an experimentally striped female, proved conclusively that the change of the color determinant must have been induced by the striped body, *i.e.*, that it was brought about by "somatic" and not by "parallel" induction.

CHAPTER XXI

CORROBORATIONS AND CONFIRMATIONS

In connection with the experiments on salamanders, there arises a disadvantage as to the method, because the latter necessarily takes a very long time. According to my experiences, the salamander needs at least three and a half years to mature sexually, and therefore it takes about the same time, or even somewhat longer, until the next generation is born.

An added disadvantage is the fact that color changes, which, in the course of these experiments, are to be acquired and to be passed on to the next generation, also develop only slowly and require years to be properly followed through. At least this is the case if one waits, as I did, for the salamanders to fully develop before exposing them to color changing surroundings. As explained before, this process may be accelerated by already subjecting the larvæ to a contrast-colored environment. In the latter case, the metamorphosed salamander emerges from the larvæ in his new coat of colors "all ready to be worn," and this makes it impossible to observe all the stages of the color changing process. Under these conditions, it is impossible to observe the change of the black and yellow pigments which, on the larva, do not show in their final stage.

On account of all the reasons enumerated, it took

almost two decades until the salamander experiments finally reached the stage as described here. And if one is unlucky in the course of following up the one or the other experiment; if, for one reason or the other, one loses the specimens of, let us say, the controlling tests already in the second generation; or if a plague strikes the terraria; or a sudden heat wave kills the experimental specimens, one has to start all over again and may rest assured that it will take at least eight more years to reach the stage where the experiments suffered an interruption.

It ought to be easy enough to imagine how a researching scientist feels who, after having gone to all these troubles, having deprived himself not only of the luxuries but of the very necessities of life in order to keep on experimenting, runs up against the cheap but nevertheless gullibly swallowed criticisms of an audience only partly informed—criticisms which are the more easy to expound, the less the critic knows about the subject in question. Fruitful collaboration and not fruitless "armchair criticism" is what is wanted, especially as the single research worker is liable to make mistakes and err in his observations. For this reason, it is necessary that his findings be tested and corroborated by other scientists; but these testing and corroborative experiments should not, as often happens, be started theoretically prejudiced and with a more or less discernible intention that nothing should be corroborated, and that the controlling experiments should yield entirely different results.

The repetition of my salamander experiments are naturally subject to the disadvantage that they will take just as long a time as the original experiments.

And the length of time necessary for the repetition of the experiments is to be blamed for the fact that the controlling tests up to now are not further than in the first generation. But as far as this first generation is concerned, my experiments have been fully corroborated, either by incidental observation or by methodical testing. While the latter experiments were conducted by Secerov (1914), v. Frisch, Przibram-Dembowski, MacBride (1923), E. G. Boulenger, and C. Herbst, first reports were made by Becker, v. Fejérváry, Gaisch, Himmer, Millot, v. Schweizerbarth, and Wiedemann.

Regarding Herbst's report, I have to remark that his dissenting deduction only seems to point theoretically to a contrary finding, as clearly explained and substantiated in the papers of v. Frisch, Przibram-Dembowski, and F. Werner.

CHAPTER XXII

THE DECIDING EXPERIMENT WITH CIONA

In spite of the results my salamander experiments had produced, I was intensely interested in finding an object which would make it possible to accomplish quicker results and which, at the same time, would permit an exclusion of a number of iniquities still inherent even to the salamander experiments. Such an object I found (Kammerer, 1920 a, 1923 a) in the Sea-squirt *Ciona intestinalis,* which had the added advantage that it hardly required any care. Contrary to this, the salamander needs much attention (Fig. 24) and, as a rule, zoölogists, while anatomically knowing all about the animals, only rarely possess the practical knowledge of how to keep them.

As I must necessarily assume that the sea-squirt is a stranger to many of my readers, I will first explain the construction of its body, as far as this knowledge is necessary to follow intelligently the development of my experiments to prove the inheritance of acquired characteristics.

The sea-squirt (Fig. 25) is anchored to the bottom of the sea by its stolons, not unlike a plant with its roots. Two siphons extend from the head of its cylindrical body—the longer is the inhalant or oral; the shorter is the exhalant, or anal, tube. The ocean water rushes through the longer inhalant tube

uninterruptedly, loaded with nourishing particles, and leaves the body through the shorter exhalant tube, together with such remnants which are indigestible. Below the opening of the inhalant tube, the digestive duct commences and leads down into the lower part of the body where it shoots upward in the form of a *U* to the point where the exhalant tube branches off. In the coil of the intestine, right at the bottom of the *U*, the generative gland is situated, consisting of a hermaphrodite gland. The latter (white in the photograph on account of the transparency of the body, Fig. 25) in reality is yellow.

If one cuts off the two siphons (the inhalant and the exhalant tubes), they grow again and become somewhat longer than they were previously (Fig. 26). Repeated amputations on each individual specimen give, finally, very long tubes in which the successive new growths produce a jointed appearance.

As a matter of course, simultaneously with the experiments, controlling cultures were maintained. In the case of *Ciona intestinalis,* specimens of the same age and to the same number (eighty to one hundred in each experiment) were kept in receptacles of the same size and under identical conditions except for the fact that the siphons of specimens of the controlling cultures were not amputated. In the latter case the siphons of all the specimens in all the generations remained of normal length, *i.e.,* decidedly shorter than those of the experimental specimens.

The offspring of the amputated specimens with their overlong siphons though never operated upon have also siphons longer than usual; but the jointed appearance has now been smoothed out. When nodes

Fig. 25

Fig. 26

Fig. 27

THE SEA-SQUIRT *Ciona intestinalis*

(Photos of living specimens by Prof. Adolf Cerny, Vienna)

25. Normal specimen

26. Specimens in the process of regenerating their amputated siphons. Some of them have already elongated tubes, as shown by the new tender growth on the old trunk

27. Non-amputated offspring (*var. macrosiphonica*) of such specimens as grew elongated tubes as a result of amputation

are to be observed, they are due not to the operation, but to interruptions in the period of growth, just as the formation of rings in trees in the winter. That is to say, the particular character of the regeneration is not transferred to the progeny, but a locally increased intensity of growth is transferred.

In unretouched photographs (Fig. 27) of two living young *Ciona,* attached by their stolons to the scratched glass of an aquarium, the upper specimen is contracted, and for this reason does not show anything significant; the lower is at rest and shows its monstrously long siphons in full extension. They were already there prenatally for the young *Cionas* were bred from parents whose siphons had become elongated by repeated amputation and over-growth.

In those animals with artificially lengthened siphons we can, furthermore, combine with the amputation at the front end another amputation at the hinder end. At the hinder end, in the coils of the intestine, lies, as said before, the generative gland. We remove the whole of this part of the body and leave the front part to grow a new hinder end and a new generative organ (Fig. 28).

Through this restitution alone, a whole theory is overthrown—Weismann's theory of the continuity of germ plasm (1892, appeared in English translation, 1915), which for decades had been accepted by the greater number of naturalists and which is to blame for refuting the heredity of acquired characteristics, by means of a sweeping conclusion. The Weismann theory insists that a germ can be generated only by another germ; that the germ plasm of every new generation is immediately derived from the germ plasm

of the previous generation. The germ plasm may generate the body, but never *vice versa*—the body generate the germ plasm.

We have actually observed this reversion, however, in the Sea-squirt *Ciona* and analogous examples are already know from almost every tribe of the animal kingdom (E. Schultz, Driesch, T. H. Morgan 1902, and others). These cases are surely more frequent than those where embryological investigations succeeded in proving the uninterruptedness of the germ-plasmic development ("*Keimbahn*"). The same can be observed on every flowerless shoot which is planted and later on blooms. All this goes to show that the continuity of the germ plasm could, at the most, be considered possible and under no circumstances as necessary.

The long-siphoned sea-squirts with regenerated germ plasm give birth to a progeny also long-siphoned (Fig. 28). I hope that in this way I succeeded in cutting the Gordian knot, not like Alexander the Great with a sword, but with a dissecting knife. The most familiar objection brought against the inheritance of acquired characteristics, the claim that there is a direct influence on the germ plasm—now, I think, is definitely removed.

The local character of the operation in cutting off the siphons renders this chief objection almost inapplicable. We might, however, still imagine that there is a method, a mechanism of directly influencing the germ plasm. While I am cutting the siphons at the head end, a direct physical reaction is taking place on the germ plasm in the hinder end. In this case there would already be established that determinant which

would give rise to an apparent inheritance in the progeny.

But now we cut away all the generative organ, with all its germ cells and its active and latent, possible and impossible determinants. We await the growth of a new generative organ. This regeneration, taking place at a time when there are no further disturbances influencing the body, is now completed. The regenerated specimens propagate themselves and the new generation appears long-siphoned. The change, therefore, cannot have been lying preformed in the original germ plasm. It can have come ultimately from nowhere, but from a body changed accordingly.

Now I am ready once more to hear what I have so often heard, "Kammerer's experiments are interesting, but, of course, they need corroboration by other scientists."

Why, certainly my experiments need "corroboration," because every research worker is liable to make mistakes which have to be corrected by others. But something lies hidden behind those stereotyped, yet justified, demands for corroboration. To repeat my experiments is a test which nobody can wish for more fervently than I. But this stereotyped demand for corroboration hides the insinuation that my experiments are not only subject to unavoidable mistakes in observation, but wrong in their entirety.

For this reason, I have taken the liberty to play a little joke on my most distinguished fellow-scientists. The last mentioned experiments with *Ciona* have already been performed in their single stages by other scientists: only their combination and the application

of the composite experiment to the problem of heredity is my mental property.

The over-long regeneration of the amputated siphons has been successfully experimented with before me by Mingazzini in Naples. The regeneration of the generative gland from somatic material was observed by Eugen Schultz, who experimented not exactly with *Ciona* but with *Clavellina,* which also belongs to the sea-squirts (Ascidians). The often-clamored-for corroboration of my individual experiments in this case was offered long before.

Researches of my forerunners received obscure publication only and are little known, so when I lectured on my *Ciona* researches before the Linnean Society of London, Professors Goodrich and Cunningham, just as expected, immediately demanded "corroboration."

Mr. Munro Fox, during the summer of 1923 at Roscoff (Bretagne), tried to duplicate my experiments in regenerating over-long siphons, but his results were negative. The amputated siphon always regenerated to its normal length only. Mr. Fox, in *Nature,* Vol. 112, p. 653, has announced an explicit treatise on his observations.

Now, Mr. Fox amputated only *one* of the two siphons, the oral or inhalant tube, while the other, the aboral, anal or exhalant tube, was left intact. This shows that he did not exactly duplicate my experiment, but, as so often happens with corroborative research work, conducted an experiment under entirely different conditions. For this reason, he necessarily had to arrive at a different result, for which my researches cannot very well be blamed. In the present case, I fortunately had a witness—Mingazzini—whose experiment

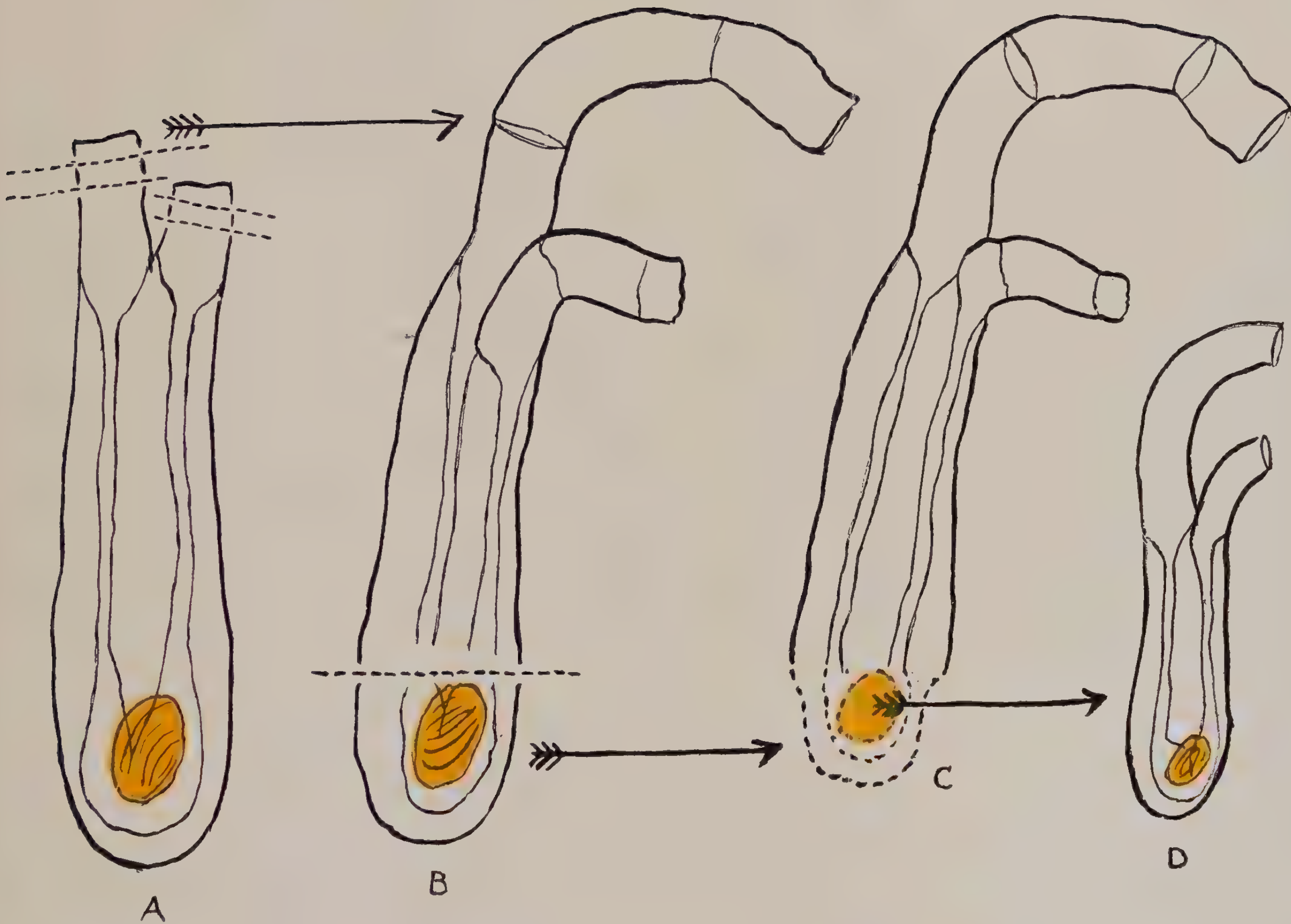


Fig. 28.—THE SEA-SQUIRT *Ciona intestinalis*
(Schematic chart)

A, Normal specimen. (The dotted lines through the siphons indicate where these were amputated.) *B*, Specimen with elongated siphons. (The dotted line through lower part of body shows where the latter were severed from the upper part.) *C*, The same specimen as *B*, with the regenerated lower part of the body including the sex gland. *D*, Young specimen brought forth from the regenerated sex gland of specimen *C*

(Original)

I corroborated. Otherwise I would again have been blamed for deluding myself or perhaps even harsher terms would have been used to criticize me.

Most probably, and in agreement with a supposition of MacBride (1923), in the experiment of Munro Fox the non-amputated siphon regulated the length of the siphon which had to regenerate. Another possibility is that the *Ciona* population with which Fox experimented, having its habitat in the North, reacts differently from the populations which Mingazzini and I used and which came from the Gulf of Naples and Trieste.

There is another thing hidden behind the demand for corroboration, which in itself is quite justifiable. One assumes that the result requiring corroboration may now safely be put off for the next five or ten or even twenty years, in favor of some other theory. In this way, one thinks to have disposed of the obligation to pay any attention to the results—which for one reason or the other seem inconvenient—before the corroboration has been perfected.

One even forgets that several stages of my experiments have already been corroborated and, because they are in accord with my results, make it the more probable that the still missing corroborations will also yield affirmative results. But ever and again I observed that corroborations were disregarded, and ever and again I heard it said, "Why, certainly, these experiments are very interesting . . . but corroboration is what we want."

Wherever my experiments were duplicated in strict accordance with the original experiment (that is, that they were actually repeated point for point) the re-

sults of those tests were always identical with my results. For this reason, I consider myself entitled to ask that my distinguished opponents will kindly remember the results of my corroborators instead of simply ignoring them until the new corroboration (which may take years, even decades) has come to a conclusion.

From all the aforementioned results and reasons, it becomes evident that, regarding the inheritance of acquired characteristics, we will ruefully have to return to the affirmative opinions of Lamarck and Darwin. Without exactly underwriting word for word Darwin's (1875) theory of pangenesis, by which he tried to make the inheritance of acquired characteristics more plausible, we come to understand that heredity, at least in principle, works along the lines laid down in Darwin's theory of pangenesis: By passing on changes of the body to the germ plasm, with the body itself playing the part of the organic mediator; *i.e.*, by way of "somatic induction."

CHAPTER XXIII

BODY AND GERM PLASM

It was not the transmission of characteristics which became hereditary and which we could observe under favorable conditions on the sea-squirt *Ciona,* but the rapid and precise reaction which distant parts of the body have on the germ plasm that became evident through ingenious experiments conducted by Schiller on tadpoles and little crustaceans. Injuries and burns immediately result in decided changes in the propagative cells, as for example, when a feeler was cut off from a *Cyclops* (Fig. 29), or when the tail-end of a tadpole

Fig. 29.—CYCLOPS

A small fresh-water crustacean. If, for example, the feeler is cut near *a* structural changes are induced in the egg sacs at both sides of the abdomen

was singed. The germ cells mature differently than under normal conditions, and the coloring matter of the germ plasm, the chromatin (Chapter I), which is

usually considered the very substance of inheritance, is differently arranged as in a case where it had suffered no interference.

Decisive disturbances in the intricate structure of the germ nuclei were observed by Stieve on newts (*Triton = Molge*), which he kept in the aquarium under abnormal conditions as far as food, temperature and light were concerned.

MacDougal succeeded in influencing various plants, among them *Oenothera* and *Raimannia,* by injecting into their pistil solutions of zinc and copper sulphate, and potassium nitrate and sugar, as well as by exposing them to radium rays. These injections resulted in occasional manifestations of changes which proved to be hereditary, in spite of the fact that the injected solutions never reached the seed. For this reason, the changes could not have resulted from a direct stimulus on the germ plasm.

However, that there is an infusion of substance from the body to the germ plasm becomes clear from very enlightening experiments conducted by Sitowski on a moth, *Tineola biselliella* (Fig. 6, Colored Plate facing p. 44); by Riddle on poultry, and by Gage on poultry and guinea pigs. In all these experiments, a harmless coloring matter (Sudan red III) was resorted to which was either added to the food, or else mixed with the bathing water, thus effecting a reddish appearance in the live animal. The coloring matter became not only a part of the body, imbedding itself especially in the fat deposits, but also penetrated to the germ plasm. This resulted, in turn, in the progeny of such animals still appearing reddish, in spite of the

fact that they were not subjected to the influences of Sudan red themselves.

These observations are especially interesting in connection with the Moth *Tineola* (Fig. 6, p. 44), whose caterpillars were fed with Sudan red. This color prevailed in the different stages of chrysalis, butterfly, and egg; and without additional coloring influence, the caterpillars emerging from the egg were just as red as their progenitors. Although in this case the artificial coloring presents nothing more than a similarity to the inheritance of acquired characteristics, it nevertheless clearly shows how heredity usually works. Such an influx of substance penetrating so the germ plasm is strictly in accordance with Charles Darwin's Theory of Pangenesis (1875), and the demands of Cunningham (1908) and Hatschek, which the latter laid down in his "theory of the generatules." It has been furthermore proven by the coloring experiments that fragments of the coloring substance are retained by the germ plasm, fragments which under natural conditions could be compared to the germ particles (genes, pangenes, determinants) which are supposed to pass on acquired characteristics to the progeny.

Nature itself supplies us with an example which in more than one respect reminds us of the artificial coloring of living beings. This example is supplied by a species of *zoöphytes*, the *Chlorohydra viridissima* (Fig. 30). This polyp is of a beautiful shade of green which is due not to its own animalic coloring matter, but to a microscopical plant, a unicellular alga in the inner stratum of the polyp. In a case where an egg is generated in the space between the inner ("entoderm") and the outer stratum ("ectoderm") the

Fig. 30.—GREEN FRESH-WATER POLYP (*Chlorohydra viridissima*)
(Length section, magnified)

te, tentacles; *m*, mouth; *t*, male gonads; *ov*, female gonads; *kn*, bud; *fp*, foot disc. The algæ inhabit the inner stratum of the body. (This illustration will serve to make Fig. 31 more explicit)
(Korschelt and Heider)

alga *zoöchlorella* immigrates the egg in great quantities (Fig. 31). The young polyp, which is about to emerge from the egg, is already provided with the necessary amount of the alga which it needs, especially for the absorption of oxygen. It does not have to ac-

quire this symbiotic growth by a new infection from the outside.

Nussbaum mentions this symbiotic dependency of alga and polyp as a striking example of an inheritance of acquired characteristics, not so much on account of foreign matter (which the alga really is) penetrating

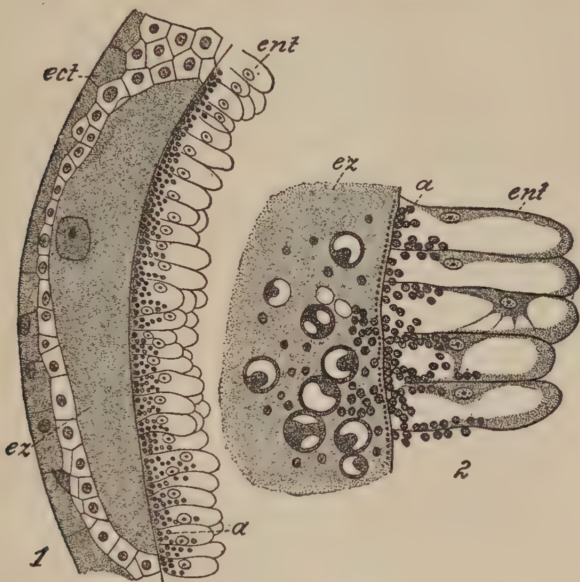


FIG. 31.—GREEN FRESH-WATER POLYP (*Chlorohydra viridissima*)
(Sections through strata of the body)

(1) Slightly magnified. Between the outer stratum (ectoderm *ect*) and the inner stratum (entoderm *ent*) an egg cell (*cz*), in the process of developing and maturing. The green algæ (*a*) inhabiting the inner stratum, collect at the side facing the egg

(2) Greatly magnified *ent* five cells of the inner stratum; *ez*, a part of the egg cell. The algæ (*a*) penetrate the egg whereby a number of algæ are destroyed (the swelled up vesicles in the picture)

(Oltmanns)

to the germ plasm, as for the fact that two living beings, so widely different—a plant and an animal—

ever and again exercise a natural influence on each other. At one time, they surely had to attach themselves to each other in order to eventually acquire this mutual adaptability. To us the penetration of the alga to the egg is exactly the same as the similar penetration and depositing of unanimate coloring matter. There is a continuous interchange between body and germ plasm, a shifting of matter which may incidentally carry with it those particles which are the very substance of newly acquired characters and which avail themselves, so to speak, of this shifting as a vehicle.

CHAPTER XXIV

GRAFT-HYBRIDS

As recently as only a few years ago, one could state without being challenged that parts of bodies, brought in close contact by ingrafting, would not influence each other as to their specific characteristics. Since time immemorial the horticulturists have availed themselves of this knowledge to improve flowers and fruit trees, the process of improving being successful because the superior characteristics of the small shoot or "scion" inserted in another tree, as the "stock" to support and nourish it, are in no way encroached upon by the inferior characteristics of the latter.

That the "scion" was in no way influenced by the "stock" was frequently drawn upon as probably evidence against the inheritance of acquired characteristics. At first glance the reader may not see any connection between grafting and inheritance, but just the same such is the case concerning the following:

If we believe in the inheritance of acquired characteristics, then the body must be able to pass on to the germ plasm the characteristics inherent to its own different parts ("somatic induction," as explained in Chapter V). To elucidate: the various parts of the body must principally be able to influence and pass on to each other organic characteristics. But lo! in the vegetable as well as in the animal kingdom, we

observe time and again that such phenomena are not the rule at all.

Tadpoles of different species of frogs (Born, Harrison), larvæ of different species of butterflies (Crampton), different species of earth-worms (Joest), differently colored specimens of the Crinoidean *Antedon rosaceus* (Przibram, 1901) and others preserve, when ingrafted on each other, their individual characteristics. Even in specimens, metamorphosing from artificially composed larvæ, it is easy enough to distinguish between, say, the front part of the Pickerel-frog (*Rana palustris*) and the hinder part of the body derived from the Wood-frog (*Rana silvatica*), as Harrison's experiment shows.

The inability of parts ingrafted on each other to exchange characteristics seemed to permit the deduction that there also is no such reciprocal effect between body and germ plasm. The germ plasm appears to play the rôle of the "scion" in regard to the body, the "stock"; or in the language of Weismann, it plays the part of a parasite. Like such a one, the germ is supported and nourished by "the stock" with which "it happens to be in contact," but just as little as the tape-worm found in the intestines of man assumes human characteristics, the "scion" would assume the characteristics of the "stock."

The independence of the scion from the stock, compared to the independence of the germ plasm from its body, becomes more obvious if a generative gland (including the germ plasm proper) is utilized for the "scion." The independence of such a "scion" even prevails in the progeny derived from an ingrafted generative gland. This progeny inherits the character-

istics of its true parents and not those of the foster-body to which the generative gland was implanted.

In this respect, the experiment related in Chapter XVII is an exception, as in this case, the *artificially* effected striped design of the foster-mother predominates in the ovary of a spotted female. We especially aimed at this experiment and that of Finkler, who exchanged the heads of insects: the only cases in which the independence of the "scion" was tested, regarding *acquired* characteristics to explain and demonstrate how all the attempts to testify against the inheritance of acquired characteristics, as far as the latter is deducted from grafting experiments, come to nought.

But, where else the transplantation is performed accurately, that is, wherever all of the generative gland is completely removed and ingrafted on some foster-body, the characteristics of the progeny correspond with the characteristics of the true parents and not with "the stock," *i.e.*, the body on which the generative gland was ingrafted and where it developed.

But even here little discrepancies become evident. In the experiment of Guthrie (as mentioned in Chapter XVII) where the ovaries of black and white Leghorn hens were exchanged, a certain percentage of white chicks displayed black spots, apparently influenced by the coloring of the foster-mother. Tests, conducted by Davenport, apparently made it possible to assume, however, that the spotted chicks originated from remnants of the old ovaries and were nothing but hybrids of white hens and black roosters (or *vice versa*). The same thing might have happened in the experiment of Magnus, who exchanged the ovaries of white and black rabbits.

To a certain extent, we are justified in drawing this conclusion, due to the frequency of other cases where the implanted, generative gland preserved its independence from the body it was ingrafted on. One of these cases is the salamander experiment (as related in Chapter XVII), in which the striped design of the foster-mother was not effected artificially, but was natural. Other cases of this kind are the following:

The eggs of the sea-squirt *Ciona intestinalis* (see Chapter XXII), even though a hermaphrodite, have to be fertilized by some other specimen, because they are immune to their own sperm cells without the application of artificial stimuli—for example, ether, according to T. H. Morgan's report. The latter, in 1910, transplanted eggs of a *Ciona* specimen into another specimen and ascertained that, by such a process, they neither lost their immunity against the sperm cells of the original body, nor acquired immunity against the sperm cells of the foster-body.

Heape, in 1898, transplanted into the body of gray Belgian rabbits eggs out of the original ovarian tubes fertilized by white Angora-rabbits, and already in the process of development. In spite of this, the foster-mother produced genuine Angora-rabbits. Regarding this experiment, it has to be taken into consideration that the transplantation was performed not with undeveloped eggs, but when the latter had already developed into an early embryonal stage. For this reason, an influence of the foster-mother on the transplanted eggs could, perhaps, hardly be expected.

Castle and Phillips, in 1909, transplanted the ovary of a black guinea pig into the body of a white specimen; after this, he crossed the latter with a white

male. In spite of this, the two young born were purely black. The offspring of a crossing of black and white guinea pigs are also black. The "black" ovary evidently does not "whiten" in the body of a white female.

Exactly the same experiment with the identical result, but with much more abundant material, was performed by B. Wiesner (1923), who resorted to the ovaries of black rats which were transplanted into the bodies of white females. Wiesner, availing himself of the latest improvement of transplantation technic (the so-called "autophoreous" transplantation, developed by Przibram, 1923 a), inserted the foreign ovary into the tube of the foster-mother. This process, because it is performed without taking recourse to sutures, hardly ever results in any manifestations of degeneration and makes sure that the eggs get in contact with the fertilizing sperm cells, in this way assuring progeny.

Although this experiment is drawn upon to help solve the problem of inheritance, the objection could be raised that an influence originating from the body cannot very well make itself felt if the ovary is not in its proper place. In reference to its ability to be fecundated and for the purposes of inner secretion (among others the development of the female sex characteristics), the ovary does not necessarily have to be in its proper place, as has been proven time and again, because these functions are independent from nervous stimuli. Regarding the accurate functioning of the "mechanism of inheritance," the question is still to be solved whether the proper connection of the generative gland with the nervous system is not necessary.

And even another, perhaps still more weighty, objection could be raised against these grafting experiments. In crossing races subject to the Mendelian Rule, the presence of a characteristic of one of the parents usually dominates over the absence of an analogous characteristic from the other parent (Bateson's "Presence-Absence Theory"). Therefore, the direct progeny of a crossing of black and white is black; the influence of the parent actually possessing the coloring pigment dominates and, in a fashion, equalizes the lack of coloring pigment of the other parent. This phenomenon may be observed when black guinea pigs are crossed with white specimens or black rats with albino rats, as borne out in the experiments of Castle and Phillips (1909, 1911) on guinea pigs, and Wiesner on rats. Similar manifestations with even more probability could be expected in reference to grafting experiments. Does it seem probable that ovaries predestined to be black are, so to speak, bleached by a body which has no coloring matter of its own? Isn't that expecting too much? Does it not seem much more possible that a "colorless" ovary would be "colored" by a body possessed of pigment?

By and large, basing one's opinion on grafting experiments related so far—be they conducted with germ plasm or with parts of the body—one could come to the conclusion that the independence of the object of ingrafting has been proven, and from this deduct unfavorably as to the inheritance of acquired characteristics.

On the other hand, this independence of the "scion" from the "stock" yields an observation not at all favorable to the theory that the body is basically different

from the germ plasm. It deprives the latter of one of its prerogatives, because the same "independence" —if there is such a one—which is demanded in reference to the relation of germ plasm and body, also pertains between any two other parts of the body. However, we know that the different organs co-operate and, in multifarious ways, are attuned to each other. Even if, as a rule—at least in transplantation cases—they do not exchange characteristics, the fate of no part of the body is genuinely independent from the other.

Added to this, "the law of independence" of transplantations—according to the latest experiences—is subject to many exceptions. This "independence" is not at all a biological law, but only a rule frequently to be observed. The following cases, however, I am not yet ready to include in the exceptions:

If, in cases of different butterfly larvæ ingrafted on each other (Crampton) in those regions where the body of one larva merges with the body of the other, the colors blend to some extent. This phenomenon may be merely the result of a diffusion of the coloring matter, and not the result of the coloring matter itself being changed while prepared by the adjacent cells. Thus, simple processes of diffusion also explain why, for example, in the case of a tobacco plant containing nicotine ingrafted on a tobacco plant almost void of nicotine, after some time, the latter is richer in nicotine (Grafe and Linsbauer) than before; or why in potato plants ingrafted with the poisonous Thorn-apple (*Datura stramonium*) atropin is found (Meyer and Schmidt).

The stimulated growth of a barley sprout ingrafted

on a grain of wheat, or the stunted growth of such a barley sprout if attached to a grain of oats (Stingl), is satisfactorily explained by the respective favorable or unfavorable food conditions. In cases like these, it is as yet unnecessary to assume an exchange of characteristics. If the eye (Uhlenhuth, 1913, 1914), the gill (Kornfeld), or the skin (Uhlenhuth 1917, Weigl) of a young salamander larva is ingrafted on an older larva, or *vice versa,* the transplanted part changes simultaneously with the whole larva, provided that the time necessary for transformation suffices. This phenomenon probably results from the better nourishment furnished by a body larger than the transplanted part; and, for this reason, the latter catches up with the development of the former. Ingrafted on a still smaller body, the transplanted part is retarded correspondingly in its development, because it is undernourished. In these cases, a specific influence on the form is not necessary to draw upon for explanation.

Regarded as exceptions of the rule of independence, most of the so-called "graft-hybrids" proved to be chimerical. For some time, botanists and practical gardeners have been familiar with quite a number of intermediate forms, which manifest themselves in the vegetable kingdom as midway stations between the original forms. These intermediate forms were not derived from sexual hybridization, but by the process of ingrafting.

H. Winkler succeeded in multiplying the number of those "graft-hybrids" by many intermediate forms which he raised from the tomato (*Solanum lycopersicum*) and the Common Night-shade (*Solanum niger*). For example: he ingrafted a branch of a tomato plant

on the stem of the Common Night-shade. He then pruned the stem and the tomato branch ingrafted on it of all the shoots that developed and which, in the one case as well as in the other, grew from their own kind. In this way, he forced the plant, right out of the region between stem and ingrafted branch, to develop shoots, the latter assuming the characteristics of both the tomato and the Common Night-shade in different combinations.

In spite of this, it soon became evident that the tissues of the tomato as well as the Common Night-shade, even as "graft-hybrids," had remained of pure strain. H. Winkler, therefore, called these hybrids *"chimeræ."* It was plainly visible wherever these two tissues grew next to each other. There were cases, for example, when the left half of one shoot clearly belonged to the tomato, while its right resembled the Common Night-shade. If "the line of demarcation" cut through the center of a leaf, half of it would resemble the lobate and split, light green, downy leaves of the tomato; while the other ovate and of darker green and smooth, manifested the characteristics of the Common Night-shade.

In such cases, it is said that the bud (that is, the zone where the growth goes on) was "divided sectorially" (about vertical to their base) and such an intermediate form is termed "sectorial chimera." Horticulture, already as far back as the seventeenth century, produced such "sectorial chimeræ" with the Italian *Bizarria,* which is a sectorial chimera of orange, citron, and lemon in which a definite line of demarcation occasionally manifested itself in the fruits, even if these grew on one and the same tree, and even a single fruit some-

times may have sections of orange, citron, and lemon.

Many more difficulties are to be surmounted to disclose the character of a chimera in those cases where the ingrafted tissues, remaining of its own kind, are not adjacent to each other, but grow on top of each other. In such a case, one speaks of the bud being "periclinically divided" (about parallel to the surface) and such an intermediate form is referred to as "periclinical chimera." Aside from a number of forms cultivated by H. Winkler and also derived from tomatoes and Common Night-shades ingrafted on each other, there are two chimeræ, for quite some time well known to the gardeners as belonging to this class of chimeræ: *Laburnum Adami,* according to Macferlane and Buder, is nothing else but the Common Gold-rain (*Laburnum vulgare*) in the skin of *Cytisus purpureus; Cratægomespilus,* according to Baur (1909, 1910), is nothing other than common hawthorne in the skin of the mistle (*Mespilus germanica*).

A new product of Winkler's (1909, 1910) "graft-hybrids," however, *Solanum Darwinianum,* up to now has not been proven as a chimera and, in all probability, is a *genuine* graft-hybrid. Where the different cells belonged is most exactly ascertained, in doubtful cases, by counting the chromosomes (see Chapter I). There are seventy-two of these in each of the cells of the tomato, while in the Common Night-shade there are twenty-four each. It has to be assumed that, for the purpose of effecting a genuine graft-hybrid, two cells, one from the tomato and the other from the Common Night-shade, must amalgamate in the same way in which otherwise only germ cells merge into each other; and in this way the chromosomes of the

tomato and of the Common Night-shade also blend. For this reason, the blended cell must originally have consisted of ninety-six (seventy-two plus twenty-four) chromosomes. This figure afterwards, by a process frequently observed in chromosomes, was reduced by one-half and so, in the body cells of the graft-hybrid, *Solanum Darwinianum,* forty-eight chromosomes are counted.

The result of forty-eight chromosomes out of seventy-two plus twenty-four of the original forms, makes the assumption of an amalgamation, a copulation of the tissues grafted on each other, plausible. Thus, the germ plasm is again encroached upon to the extent of one more of its prerogatives, because not only germ cells, but occasionally cells of other parts of the body, prove their ability to amalgamate for the purpose of fertilization and so give the start to new developments, the inception of a new individual.

The real exceptions of the rule of independence of transplantation are, for the present, no longer limited to the genuine graft-hybrid *Solanum Darwinianum.* Especially within the realms of the animal kingdom, numerous cases of "graft-hybridal" forms are known.

A masculine-inclined body acquires feminine characteristics as soon as it is ingrafted with ovaries, while a feminine-inclined body acquires male forms and urges, when ingrafted with testicles (Steinach, 1913). Also the transplanted sex glands do not remain unchanged; their generative tissue is reduced, at least for the time being, thus offering greater space to the interstitial tissue (see Chapter L).

Single parts of the body transplanted to an individual of opposite sex acquire the form of the foster-

sex. The male of the Crested or Large Eft (*Molge* = *Triton cristatus*), at the time of mating shows a scalloped crest along the center line of the back. This crest is dark brown, just as is the whole breadth of the back of the animal. In the female, the center line of the back has a rut-like indentation and is frequently bright yellow in color. If these bright yellow stripes are ingrafted on a male whose skin was removed along the center line of the back, preparatory to the transplantation, he will develop a high scalloped crest (Bresca) during the next mating season, while retaining the sulphurous yellow color.

The ventral side of the Mountain Eft (*Molge* = *Triton alpestris*) is uniformly colored orange-red. Taube, after skinning the leg of an eft, fitted a piece of this orange-red colored skin over it. Here the skin of the belly changed into skin as generally observed on legs, in reference to color as well as to the form of the skin glands. Experiments yielded identical results when Taube, instead of the leg of the Mountain Eft which was employed to furnish the skin of the belly, resorted to legs of the Great Eft (*Molge cristata*) or to those of the Small Eft (*Molge vulgaris*) to ingraft the skin on.

The exchanges of eyes, as performed by Koppányi (1923) on different vertebrates, as well as the exchanges of whole heads as performed by Finkler on insects, proves the reciprocal influence of colors between the ingrafted part (scion) and the body into which it is transplanted (stock). The torso of the insect on which a foreign head was ingrafted changed its color to that of the insect which furnished the head. The yellow-bordered water beetle (*Dytiscus*

marginalis), for example, with the head of the large black water beetle (*Hydrophilus piceus*) loses the yellow outline of its wing colors and becomes pitch black. Burbank reports similar experiences in reference to vegetable graft-hybrids. Among others, one of an annual plant which, ingrafted on the stock of a perennial plant, acquired from the latter a correspondingly lengthened span of life.

As shown in all the preceding chapters, the breeding experiment—this indispensable means of testing all questions pertaining to inheritance—could not very well be resorted to, to contradict the inheritance of acquired characteristics. To produce contrasting proofs, recourse was taken to an entirely different realm of biological methods: grafting or transplantation. We have followed our opponents even as far as this, and looking over the result of our investigations within the realm of graft-hybridization, we cannot see that any phase of it can be made out as a principal contradiction to the inheritance of acquired characteristics. There is an established ability of living substance to transmit to others the characteristics of locally limited parts of the body, be they adjacent or distant. But this ability—exactly as the inheritance of acquired characteristics has proven in breeding experiments—works only in certain cases, under strictly limited conditions, which still offer a wide field for the investigator.

CHAPTER XXV

XENIAS AND TELEGONY

If one fertilizes the pistillate (or female) flowers of a variety of maize producing yellow kernels with the pollen of a variety yielding blue kernels, the ears of the crossing-variety will contain blue kernels. In the sense of Mendel: blue is "dominant" over yellow.

The single kernel of maize consists of the embryo and the endosperm, the nourishing tissue. Only the embryo originated from the fecundated egg cell, and, until lately, it was generally assumed that the endosperm was exclusively derived from the "mother." But in spite of this, it manifests itself endowed with the paternal characteristic of the blue color. This color was apparently acquired by the endosperm by transmission from the embryo. Such a process would once again assume "somatic induction" as an hypothesis for the inheritance of acquired characteristics.

Phenomena like those observed in crossing differently colored maize are called *"Xenias," i.e.,* "a present given to a guest." What a triumph for the opponents of the inheritance of acquired characteristics when the hypothesis of relayed transmission of characteristics, in reference to the development of xenias, turned out to be wrong! The fact was established that the endosperm also contained paternal substances.

The fecundation of flowering plants (and this in-

cludes maize) is a double fertilization. When the prolific powder comes in contact with the stigma, it develops pollen tubes. Each of these pollen tubes, by way of the style, admits two male cells (spermatozoans) to penetrate to the seed. One of the sperm cells enters the egg cell, while the other blends with one of the other cells into which the seed, until the moment of fecundation, has been divided. Both fertilized cells are now stimulated to development; a division of cells sets in. But only the egg cell generates a genuine embryo, while the second spermatozoan develops into a mass of cells which furnish the nourishment of the privileged embryo. This shows that two embryos develop in the seed, both derived from fertilized germ cells, with the one embryo remaining rudimentary and gradually developing into the endosperm. No wonder if both of them—both endowed with paternal substances—manifest identical paternal characteristics.

However, if the xenias within the vegetable kingdom turned out to be something of a detriment to the inheritance of acquired characteristics, genuine xenias are furnished by the animal kingdom, even though their discoverer, A. v. Tschermak, does not exactly want to assent to this.

If a female canary bird be crossed with any other species of finches—for example, Goldfinch (*Carduelis elegans*), Greenfinch (*Chloris chloris*), Siskin (*Chrysomitris spinus*), Bullfinch (*Pyrrhula europæa*)—the shells of such eggs as are expected to produce hybrids not only show the characteristic design of the mother variety, but also clearly that of that species which contributed the male to the crossing. The egg shells

are produced by special glands inherent to the oviducts, while the egg, on its way out of the ovary to the outside, glides through the former. The exterior covers of animal egg cells (including the hard shell of the egg) are exclusively derived from the mother's body. Here, as far as is known, no double fertilization takes place. Just one single spermatozoan penetrates into the egg.

Now, if the exterior egg covers, exclusively derived from the mother, show paternal characteristics, then within the animal kingdom these are really to be considered "xenias." The term "xenias" should logically be stricken out of the vocabulary of the botanist, and be applied only to animal xenias as discovered by A. v. Tschermak. But if one insists upon abiding by the rules as accepted for the nomenclature of natural history, then the term "xenias"—first coined for manifestations in the realm of botany, whether they retain there the original meaning or not—can never be applied to zoölogical phenomena.

I deem it my duty to point out here a distant possibility that these xenias may also be revealed as spurious. In the course of every copulation, millions of male sperm cells penetrate the sex organ of the female. But only one at each time serves to fertilize one egg. What happens to the other sperm cells? It is supposed that they deteriorate and either are dispelled or else are absorbed by the surrounding tissue. In the latter case, they would serve to nourish the female organism but would not effect any structural results.

But now Kohlbrugge insists that the spermatozoas, when still alive, actively penetrate the female tissue.

If Kohlbrugge is right, then there would be a possibility that the glands of the oviducts (those which produce the egg shells) are immediately influenced in the direction of paternal characteristics. Then, also, animal xenias would be impossible; and the hypothesis of any relayed transmission would have to be excluded once and for all from this realm of investigation. But now, while the theory of the inheritance of acquired characteristics is robbed of a support, it is strange to notice that, just by refuting the xenias, an ancient belief, mostly prevalent among breeders, is resurrected, which supports the theory of acquired characteristics, namely, belief in the so-called telegony.

The theory of *telegony* supposes that every fecundation produces an after-effect noticeable in later fertilizations. For example: If a female shepherd dog once copulated with, say, a bulldog, then not only the progeny of this copulation, but all her later progeny, even if procreated with a male dog of her own race, will to a certain extent display characteristics of the bulldog. For this reason, many breeders consider a female definitely depreciated in value as soon as she has only once been fertilized by some other breed.

This belief, hard to reconcile to our present-day conception of fertilization, was already considered disproved; especially since Ewart offered evidence of its errancy in reference to domesticated horses and zebras. A mare once fertilized by a male zebra, later on gave birth to foals which showed a slight inclination to manifest stripes on their legs, in cases where a stallion of the usual kind had been the sire. But later on, it became evident that stripes of that kind are frequently

to be observed in foals in cases where a fertilization by zebra males was out of the question.

Again, it was left to A. v. Tschermak to found telegony on a new basis through his crossings of female canary birds with males of different species of finches, and by crossing of different breeds of poultry. As mentioned before, it is remarkable to notice that the egg shell shows paternal characteristics. If a hen, usually laying white-shelled eggs, is fertilized by a rooster of a race producing brown eggs, the shells of the eggs produced by the hen from then on will be of a brownish color, even though, in future, the hen copulates exclusively with males of her own kind that produce only white-shelled eggs. Analogous to this, the eggs of a canary bird which were once fertilized by a bullfinch, later on showed the spots and stripes characteristic of bullfinch eggs, in cases where a canary male was the father.

I wish to make it clear right here that A. v. Tschermak, who discovered all these highly interesting phenomena, is not inclined to interpret them as relayed induction, thus favoring the inheritance of acquired characteristics. Rather, he takes pains to explain that such an interpretation of his findings "as a matter of fact" were out of the question. Because "as a matter of fact" there is no such thing as an inheritance of acquired characteristics! A deduction which in our days has apparently become a thing to be taken for granted.

At any rate, in A. v. Tschermak's experiments, we witnessed phenomena which hardly fit in with our present conceptions of fertilization and inheritance. We have to expect that these important observations will

shake the very foundations of a number of other scientific dogmas, not alone the dogma of the non-inheritance of acquired characteristics. Should this not teach us to refrain altogether from dogmatism in science?

CHAPTER XXVI

WHY MUTILATIONS ARE NOT HEREDITARY

THE belief in the inheritance of acquired characteristics which, until the ascent of Weismann (1882, 1892), was accepted as a matter of course was originally shaken to its very foundations by the experience that mutilations are not inheritable. More than through his theory of continuity of the germ plasm and the deductions derived from it, Weismann undermined the belief in the inheritance of acquired characteristics through one single, noted breeding experiment. This experiment consisted in Weismann (1892) cutting off the tails of twenty-two consecutive generations of white mice shortly after birth, with the result that the newly born mice ever and again had tails of normal length. On this account "many Lamarckians silently retreated," as Bode puts it. As a matter of fact, there was little reason for these Lamarckians to silently retreat.

In the first place, a mutilation does not constitute an acquired, but rather a "lost" characteristic. Even if one insists that a mutilation be considered an acquired characteristic, it seems quite hasty to deny the inheritance of acquired characteristics as a whole on account of nothing else but the non-inheritance of a single characteristic, especially if the latter belongs in a strictly limited and mechanical area. Since Lamarck,

Fig. 32.—JAPANESE DANCING MICE

Upper picture: Mother with mutilated tail. Lower pictures: Two young, also short-tailed. The dotted line shows the normal length of the tail. (This case may not without reserve be claimed as heredity of mutilations)

(Hammerschlag)

who believed in the inheritance of "all characteristics acquired in the life of the individual," nobody else has ever dared to make such a sweeping statement.

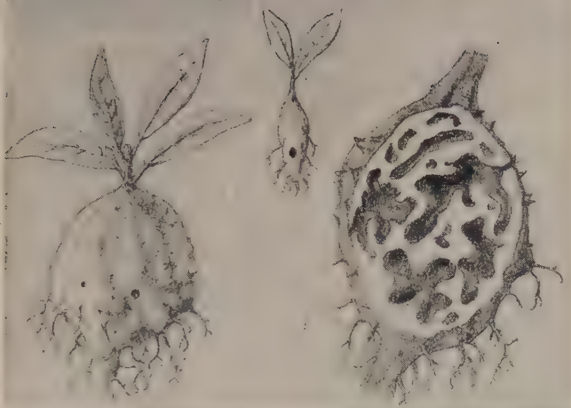
In the second place, mutilation is not a genuine *characteristic* of a living being. According to Semon (1912), an organic characteristic can consist only in the way in which a living being reacts on an influence, but the only possible reaction to mutilation is a healing of the wound—if possible, a restitution of that which has been lost. The reaction to mutilation is regeneration. The mutilation itself is the influence on which to react, but not a characteristic by itself to be passed on. What, after mutilation, can be inherited is clearly shown in the example of the sea-squirt *Ciona intestinalis* (Chapter XXII). In that case, something was inherited which had developed on account of the lost organ, *i.e.*, the intensified growth of the regenerated siphons.

But in many other cases, the parental organism itself is not able to effect such a substitution. Either the new growth is smaller in size than the original growth or it does not regrow at all. The latter was the case in reference to Weismann's experimental animal. Even the newly born mouse is unable to restore a cut-off tail. The embryo at an early stage, however, can regrow the tail. The embryonal development is nothing else but a comprised repetition of all the different stages of the whole process of evolution. At the "sea-squirt stage," if the embryo of a higher animal (mammals, man) had to pass through such a one the latter would have been able to regrow all that which a fully developed sea-squirt can regrow. At an earlier stage, their ability to regenerate would have

been even correspondingly greater. To make it quite plain: The loss of an organ that the fully developed animal cannot recover can no longer be detected in the progeny, as the latter equalized this loss during its period of development.

Only in a case where the loss occurred shortly before the moment of conception (*i.e.*, that the germ was not given enough time to regenerate the substances it was deprived of) the reappearance of a defect (loss) could be not entirely out of accord with our biological understanding. And just this reasoning tends to make clear how incomplete and lacking in strength of evidence are the mice experiments of Weismann (1882, collected in 1892) and even the later experiments of Ritzema Bos. He should not only have cut off the tails of young mice before they were sexually mature, but he should also have deprived fully matured animals of their tails, as shortly as possible before conception and fecundation. In an experiment like this, which more accidentally than purposely was conducted by Hammerschlag on dancing mice (Fig. 32), the young of mutilated parents were actually born with mutilated tails.

How far we still are from definite findings in this respect becomes clear from the grafting experiments conducted by Hermann Braus on the fore legs of tadpoles. As mentioned in our succinct description regarding the development of frog larvæ (Chapter VIII, Fig. 7), the tadpoles first grow their hind and then their fore legs. That the fore and the hind legs do not appear at the same time is not a result of a late development of the fore legs. The four extremities develop simultaneously, but the fore legs remain be



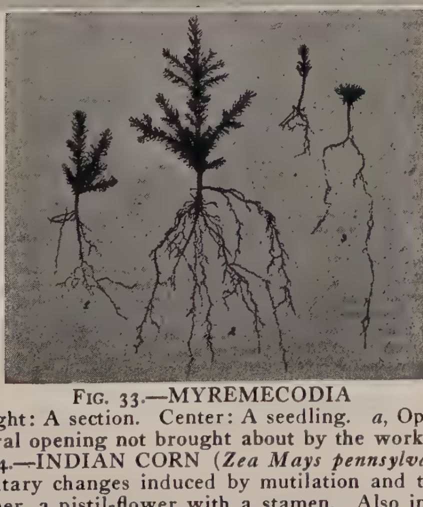


FIG. 33.—MYREMECODIA

t: The entire plant. Right: A section. Center: A seedling. *a*, Opening to the inner cavities. *b*, Natural opening not brought about by the work of ants

FIG. 34.—INDIAN CORN (*Zea Mays pennsylvanica*)

reditary and non-hereditary changes induced by mutilation and twisting of the stalk. Note he lower left-hand corner, a pistil-flower with a stamen. Also in the same row, on a black kground, the inverted case: stamen-flowers whose husks, to an extent or altogether, have n changed into stigmas. Finally, a small ear belonging to a new, early maturing race (*Var. præcox*).—(Blaringhem)

FIG. 35.—PINE (*Picea excelsa*)

ree years old, raised from seed at an altitude of 600 feet. The seed employed (from the left he right) was collected at 4,200 feet and at 2,400 feet in the Achental (Tyrolean Alps), and the two smallest specimens in Finland.—(Cieslar)

neath the skin which covers the gills ("operculum"); when they are fully developed, they finally push through the skin. This gives the impression that force had been applied, and very often this pushing through of the fore legs first occurs on one side—usually the right—and only then on the other side. The torn skin which covered the gills now has the appearance of a tattered sleeve from which an arm protrudes.

To make sure that the arm is able to push through by sheer force of its own, Braus transplanted the yet undeveloped fore legs under the skin of the back, where they grew just as though they were in their proper place and pushed through the skin at the proper time.

But what happened on those spots where normally the fore legs pushed through the skin? Without the legs pressing against it (for the legs had been transplanted to the back of the specimen) the skin became thin and transparent. In many cases—though not all—there appeared a hole in the center of the transparent skin, which was smaller than the hole normally pushed through by the fore legs but clearly enough conceivable and easily enough felt with a sound.

Braus himself came to the conclusion that the pressure which so many generations brought to bear upon the skin and the subsequent tearing of it, resulted into a *mechanomorphosis* which finally became hereditarily established without pressure, even though the effect therefrom was diminished in force.

The same theory may apply to the unique growth of *Myrmecodia* (Fig. 33), *Myrmedoma,* and *Myrmephytum,* on the trees of the primeval forests of Ceylon

and the Malayan Archipelago. These plants possess a thorny, potato-like, broadened, very short stem which consists of a labyrinth of small nooks and passages which are inhabited by ants. It was always supposed that the ants themselves burrowed these nooks and passages and therefore were responsible for the puffed-up appearance of the plant.

Young plants of *Myrmecodia echinata* raised from seeds at the Botanical Garden at Prague, Czecho-Slovakia, and undisturbed by tropical ants which were not present in the hothouse, developed these very same nooks and passages observed in the plants growing in their proper habitat. From this, it was rather hastily deducted that one was not dealing with an adaptation to the ant, but that the ants simply availed themselves of this convenient and ever-present habitation. My deduction is different. I hold that the first mentioned theory is more substantial: exterior as well as interior development of the *Myrmecodia* were affected by the ants. By continually burrowing nooks and passages into the different generations of *Myrmecodia,* the ants finally succeeded in forcing the plant into a new form of development. After having worked on so many generations of *Myrmecodia,* the ants need no longer exert themselves, because now the plant grows in accordance with the necessities of the ants. Mechanomorphosis has become hereditary with *Myrmecodia.*

As additional material for the correctness of the theory thus developed in reference to the *Myrmecodia, Myrmedoma,* and *Myrmephytum,* may be considered the fact of their parasitical growth on trees. Today this has become general, but it may have been originated by the habit of the ants to carry seeds up

the trees, there to sow it into humus, also carried up to the trees. Many plants of these "hanging gardens," in spite of originally growing on the ground, eventually make their homes in trees and there adapt themselves to their epiphytical habitat. This most probably also applies to the *Myrmecodia* and thus the assumption that all the other proofs of adaptability originated from symbiotic relations between ants and plants is the more substantiated.

CHAPTER XXVII

THE INHERITANCE OF MUTILATION CONSEQUENCES

ALL this should go to show that not even the question of the inheritance of mutilation (supposedly long since disposed of in a negative sense) has been completely and satisfactorily explained. On the contrary, additional experiments in this respect seemed very desirable. In general, however, we may assume today that not mutilations, but their results can be inherited, because they constitute a stimulating reaction of the mutilated living being.

In this respect, the sea-squirt *Ciona intestinalis* (Chapter XXII) amounts to a convincing proof. Another convincing example is presented to us in the experiments conducted by Blaringhem (1907; 1911) on maize (*Zea Mays Pennsylvanica.* Fig. 34). Blaringhem either cut off the main stalk or twisted it. This resulted in the mother plants assuming various abnormal forms which were partly repeated in the young plants, the latter taking on hermaphrodite forms for the most part. Normally, maize is hermaphrodite inasmuch as the stalk is topped by a panicle which consists of male blossoms (stamens) only, while the ears shooting out of the stalk at intervals are made up from female blossoms (pistils) only. This shows, then, that maize is hermaphrodite as a whole, while the panicle and the ears each are of different sex. On

account of the mechanical interference by Blaringhem, panicle as well as ears each became hermaphrodite: between the corns on the cob, stamens grew; that is, male organs appeared among the female, while the husks of the male panicle developed stigmens and thereby changed into female conceptive organs. Both these changes proved to be hereditary and, aside from this, a decided tendency to prematurity became evident, as well as a differentiation in the structure of the stalk and the number of the leaves.

Blaringhem (1908) also succeeded in proving heredity of mutilation results on barley, mustard, and spinach; Klebs on Speedwell (*Veronica chamædrys;* 1906) and on Houseleek (*Sempervivum acuminatum;* 1909).

CHAPTER XXVIII

INHERITANCE EXPERIMENTS ON PLANTS

To prove even more convincingly that the same law, which up to now we observed as prevalent in the animal kingdom, also applies to the vegetable kingdom as a whole, another example for the inheritance of acquired characteristics from the realm of botany may be resorted to. For this example, I avail myself of the experiments which Cieslar conducted with pines (*Picea excelsa,* Fig. 35) whose seeds had been gathered in at different heights—at the crest of the mountain (4700 feet), at the intermediate region (2500), and in northern climes. These seeds were sown in experimental beds adjacent to one another and at the same height over medium sea level (630) in a botanical garden. The young plants did not develop at the same rate, even though the seeds were sown at the same time. What happened was this: the young plants raised from the seed of the intermediate region developed faster than those raised from seeds of pines whose habitat was the crest of the mountain. The young plants raised from seeds of the pines which grew in the northern climes developed even more slowly, for the simple reason that their mother pines grew slowly on account of the inclement weather. The others developed the faster the more mild the clime in which their mother plant grew.

The hereditary difference in the development of these pines seems proven by this. In spite of the fact that the three different kinds of young plants were raised in the same clime, they had retained the differences regarding the speed of growth which they had inherited from the habitat of the mother pines. Of course, even though objections will hardly be raised against the claim that the young plants inherited their respective speed of development, we should not lose sight of the fact that the assumption that the differences in the speed of growth was acquired by climate is not exactly proved, but rather substantiated by the hypothesis which, to be sure, seems quite probable.

In Europe, the peach tree (*Prunus persica*) sheds all its leaves in the fall and remains bare during the winter, growing new foliage in the spring. In the tropics, however, the peach tree is verdant throughout the year. If seeds of European peaches are sowed on the Island Réunion, young trees which grow therefrom shed their entire foliage every winter regularly, even after ten years, in spite of having grown in the tropical clime. Later on, this change of foliage becomes irregular, the period of bareness is shortened. Only after twenty years, a condition is reached which Bordage calls *"subpersistance du feuillage"*; *i.e.*, a condition when complete bareness no longer manifests itself. If seeds of the trees which have been developed into ever-green specimens are sowed, the young trees developing from these are immediately almost as verdant throughout the year as the mother plants, even when they were grown not in the hot lowlands along the coast, but at a height of three thousand feet above

medium sea level where all the other peach trees are subject to an entire periodical change of foliage.

Therefore, on the basis of the examples supplied here, we may assume that the inheritance of acquired characteristics has been proven, and this proof seems valid as regards the animal and the vegetable kingdoms. To these kingdoms of living nature today is usually added a third one—the realm which intrinsically furnished the foundation, or root, out of which grew the two kingdoms of the animals and vegetables—the kingdom of the protista. Is the inheritance of acquired characteristics also prevalent here?

CHAPTER XXIX

INHERITANCE EXPERIMENTS ON PROTISTA

THE realm of the protista is comprised of all those lowest living beings which, during the entire course of their lives, consist of only one cell. A cell constitutes the smallest amount of living matter which, as far as is known, can lead an independent life by itself as a so-called "elementary organism." In the main, the cell consists of body and nucleus, and once in a while it is covered by a membrane.

Cells of the lowest stage of development multiply only by a subdivision (fission) of themselves into two or more fragments, which in turn grow into a complete cell, and again multiply themselves by subdivision of themselves. In protista of higher development, after having undergone the non-sexual cell division of themselves hundreds or thousands of times, two cells finally merge into one, this merging amounting to sexual intercourse.

These protista up to now have not been resorted to as subject for experimental changes as they rightly deserve. Those, however, which cause diseases (*Bacteria* and *Trypanosomes*) have been studied more thoroughly, because medicine has been strongly interested in them, on account of the physiological effect of their poisonous metabolism ("virulence"). But also on account of their build, these bacteria and trypanosomes

are capable of many changes towards developing (as natural and experimental formations) new races and species. Among those who have done notable research work in this respect in the last few years, I wish especially to mention the results achieved by H. Braun, Wolf, Jollos and Jennings (1920, 1923).

The proof of the inheritance of acquired characteristics in reference to the protista, aside from all the other objections (which experiments in this respect usually run up against in the animal and vegetable kingdoms), is complicated by the obstacle of their non-sexual (vegetative) method of propagation. Most of the naturalists consider this method of propagation as something basically different from the bisexual method of propagation. Only that which has been transmitted to the progeny by sexual propagation is considered by these scientists as "rightfully inherited," and may be referred to in the relation of "progeny" to progenitors. All such individuals, however, as are engendered by division are considered one unit, analogous to a plant that grows buds which remain part of the plant and, together with the mother plant, amount to only one individual.

It would lead us much too far to elaborate on what a frail basis this differentiation in principle between vegetative and sexual propagation is founded, and how impotent that limitation of the conception of heredity is which claims that heredity can only pertain where sexual propagation prevails. In my *Allgemeine Biologie* (1920 a), I have taken occasion to prove that vegetative and sexual propagation are based on the same essential processes, as for the same reason are all the essential processes of inheritance. Supported

by these assumptions (see also Ruzicka, 1914), I am now going to relate an example regarding the inheritance of acquired characteristics on protista, which, as it may be considered as absolutely valid, opens a general alluring view.

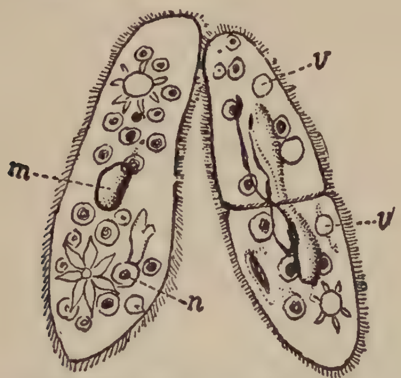


FIG. 36.—SLIPPER ANIMALCULE (*Paramæcium*)

Two specimens. The one at the right (note constriction) shortly before division, *m*, cell-nucleus (*Macronucleus*); *n*, vacuoles containing food; *v*, pulsating vacuoles. (This illustration will serve to make Fig. 37 more explicit)

(Pfurtscheller)

Jennings (1908) and MacClendon, in their protista experiments on the Slipper animalcule *Paramæcium* (Fig. 36), succeeded in developing in the latter a tendency to incomplete cell division. While generally the paramæcium propagate by a complete latitudinal separating of the cell, now the separation amounted only to a notching (Fig. 37), the different parts remaining together, gradually forming an ever-growing chain. Thus, whole string-like cell colonies were created which wind their worm-like way, and only once in a while a single cell parts from the string gradually to develop into a whole string again.

There are a number of chemical as well as mechanical conditions which will result in this incomplete division of cells; such as dirty water, hunger, and a pressure which extends from the center to the periphery and which is brought about by subjecting the container with the paramæcium to a slow centrifugal movement.

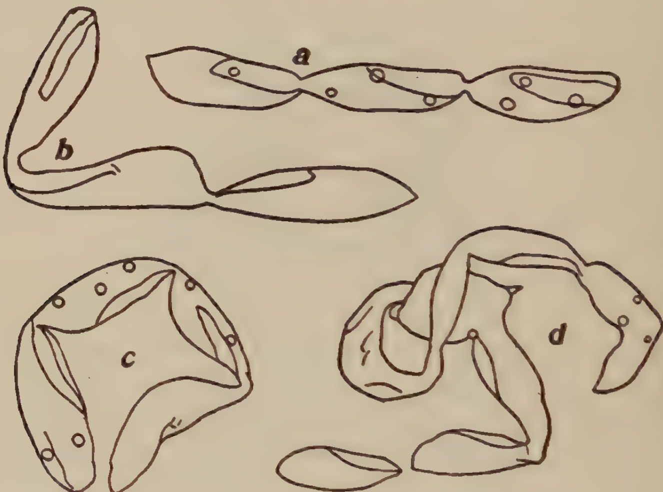


FIG. 37.—SLIPPER ANIMALCULE (*Paramæcium*)

Chains of individuals resulting from an insufficient separation on occasion of propagation by division of cells. *b, c, d,* curling in a wormlike fashion. *d,* a single cell which has separated from the chain. (*Cf.* Fig. 36)

(Jennings, 1908)

All these influences which resulted in the development of string-like cell colonies, generally unknown in the paramæcium, may be eliminated, *i.e.,* the paramæcium may be resorted to clean, quiet water and rich food; but, in spite of this restitution to normal conditions, the separation of the cells does not manifest itself again and the mere notching of the cell remains.

CHAPTER XXX

EVOLUTION OF HIGHER BEINGS THROUGH ACQUIRED CHARACTERISTICS

As mentioned in the previous chapter, protista are such living beings as, for the whole duration of their lives, consist of one cell only; they are monocellular. And this is their most striking difference from the "higher" living beings (plants and animals, the latter including human beings). All these are living beings consisting of many cells; that is, they are multicellular. All this goes to show that monocellulars (protista) and multicellulars (plants, animals, and human beings) contrast strikingly with each other. Regarding the latter, every "individual" as a matter of fact amounts to a whole colony of closely connected billions and trillions of cells in unison, forming a well composed commonwealth, so to speak. Multicellulars also commence their lives as unicellulars, because, as mentioned in Chapters XVI and XXVI, every individual for himself has again to undergo summarily all the stages of the process of evolution which finally culminated in his species. Every plant and every animal develops out of a single cell—the fertilized egg. Although this primary cell, the fertilized egg, just like protista, multiplies itself by fission, the different fragments do not part definitely but remain in connection with each other, for the purpose of mutual protection and interchange

of substance. In this way, out of the single, primary cell develops an enormous cell complex—the multicellular "individual."

We have already noticed a certain start for a development in this direction in the experiments on Paramæcium, as conducted by Jennings (1908) and MacClendon (1909). Originally, by mechanical or chemical means, but later on even without, string-like colonies of cells developed. This, of course, is only a spurious and not a genuine development from monocellular to multicellular, from a simple protozoan to a more intricately constructed living being. Just the same, this experiment seems quite a propitious beginning, because it permits us quite logically to conclude how the deciding change from monocellular to multicellular beings has once come about.

In nature, also, we find certain mechanical and chemical agents which necessitate a halt in the otherwise threatening decomposition of a young "cell commonwealth." So we find complexes of cells, each cell still separated from the other, but the whole of them imbedded in a gelatin-like matter which they commonly secrete. Such procedure can be observed on the *Polycyttaria* (belonging to the *Radiolaria*) and on certain *Melethallia* (belonging to the *Algæ*). Others like the *Volvocales* (belonging to the *Flagellata*) are not only kept together by the gelatin-like matter, but also by strands of living matter which extend from cell to cell. Closely fitting membranes, covers and shells of different description, as well as putty-like agents (calcium) are employed for the purpose of keeping the cell complex closely together.

But in spite of these provisions, beginning about

with the *Volvocales* and extending to the vegetable as well as to the animal kingdom, it becomes apparent that the aggregation of the cells is already hereditarily fixed. Without the inheritance of acquired characteristics, the important step from the monocellular being to the multicellular would have been impossible. Without this progressive method and inheritance of the new, it would have been impossible to evolve the higher developed kingdoms of the plants and animals. Without this inheritance, the development of blossoming trees and the final evolution into the "crowning glory of creation"—man—would have been stifled right in the beginning.

CHAPTER XXXI

HEREDITARY DEGENERATION AND HEREDITARY REGENERATION

SPEAKING about the hereditary aggregation of cells, which was the incentive to an enormous ascent, let us remember that already in Chapter II it was stated that the acquisition of hereditary characteristics may amount not only to progression, but to retrogression as well. Conditions of life and the way we live may tend not only to hereditarily regenerate, but also to heavily degenerate, thus seriously handicapping the progress of evolution towards an ever higher state. Examples for this up or down have already been offered in the course of this book. We need only remember the case of the Midwife Toad (Chapter IX) which partly progressed by the raising of the young, while other specimens, by abandoning the raising of its young, retrogressed and regained certain atavistic characteristics.

In general, however, in examining the material submitted so far, we did not stop to consider whether the acquisition of hereditary characteristics meant progression or retrogression. For this reason I am under obligation to my readers to furnish a number of examples especially suited to demonstrate that a development in two directions is always possible, and that it depends on

the conditions of life whether a certain organism will develop in a regenerative or in a degenerative direction.

An example proving degenerative development is found in the breeding experiments which Kapterew conducted on the water-flea *Daphnia pulex* (Fig. 38), a little crustacean of our fresh-water ponds. Kapterew kept the latter in the dark, causing their eyes to lose

FIG. 38.—WATER-FLEA (*Daphnia pulex*)

At the right, a complete specimen. At the left, six heads, showing different stages of the deterioration of the eye, on account of being kept in the dark

(Kapterew)

their regular shape and become ragged at the edges. Bigger and smaller fragments of the black coloring matter of the eyes separated and distributed themselves all over the body, eventually to vanish entirely by absorption. In the beginning, the destruction of the visual organ was more or less accidental, but in the fifteenth month this condition gradually spread to all

the specimens, and it had become hereditary; for quite young Daphnia of only four and five days displayed almost entirely discolored, and therefore destroyed, eyes. Here we are dealing with a retrogression, a degeneration whereby the evolutionary value of an organ or an organism was hereditarily reduced. To be sure, in spite of this loss, the organism need not lose in value, because organs sensitive to light are valueless anyhow to any creature living in the dark. As a sort of compensation, all the other senses, especially the sense of touch, usually develop to a finer degree.

If the loss of the visual sense on *Daphnia* kept in the dark is more or less a characteristic of adaptability and, regarding its evolutionary value, open to two different interpretations, the inheritance of artifically effected defects of the visual organs of rabbits, through nine generations as described by Guyer and Smith, permit only one equivocal interpretation, and have to be accepted as proofs of a degenerative, retrogressive development.

As an exact contrast to the loss of eyesight on *Daphnia* and eyesight defects on rabbits, my own success on the newt *Proteus* (Fig. 39) would have served best (Kammerer, 1912 b, c; 1913 g; 1920 d). Proteus is blind by nature, because it lives in dark subterranean caves. By alternately exposing the animal to artificial (red) light and daylight, I succeeded in redeveloping their eyes and restoring their visual faculty. But I observed this progression only in one generation, for I did not breed a second one. I do not doubt, however, that a second generation would again have grown big eyes, but I abandoned my research work in this direction after having proven only adaptability and without

FIG. 39.—THE NEWT PROTEUS (*Proteus anguinus*)

Two specimens, each five years old. At the top, a normal specimen raised in the dark, colorless and blind. On the bottom, a specimen raised in red light alternating with daylight. Somewhat pigmented with well developed eyes

(Original)

having proven heredity. I have taken occasion before (Chapter XIII) to explain why I abandoned this research work: I felt hampered on account of the ever and again reiterated, stereotyped objections against the inheritance of acquired characteristics which, especially in the case of the Proteus, would have been so easy to raise.

But even this resigned attitude of mine failed to stop the opponents of the inheritance of acquired characteristics from availing themselves of my Proteus experiment for an alleged refutation of inheritance. Is it possible that such a hopeless blindness of fanaticism can prevail? The blindness of the newt is nothing compared to the blindness of those "who will not see," as a well-known adage so fittingly says. Because I succeeded in eliminating the blindness of Proteus by light, it was concluded that the blindness would not have been hereditarily established. Otherwise, it was said, it would be impossible to believe that by keeping Proteus in the light for one generation only, the collective influence of so many generations that had lived in the dark would have been turned to no account.

To this I have the following reply: By keeping Proteus in ordinary daylight, it is impossible to eliminate the result of its having lived in the dark. Kept in the daylight, the skin covering the rudimentary eye of Proteus becomes filled with pigment. This considerable, but by no means complete, absorption of light is sufficient in itself to check the development of the eye, and results in the usual retrogression of the visual organ. Red light, however, to which the newt was exposed during the day, and even during the night, to intensify the effect of illumination, does not result in a

pigmentation of the skin any more than a photographical film would be blackened by the red light of the dark room. Only under the influence of this pure, chemically ineffective illumination is the retrogressive tendency of the visual organ of Proteus finally overcome.

The misinterpretation of this experiment gives me occasion for a more general remark. To test whether acquired characteristics are inherited, it seems only logical to allow a living being to first acquire this characteristic—*i.e.,* inborn characteristics have to be changed first. With the objection raised, however, against the regenerative development of the eye of Proteus, our opponents deprive us of this right. After I had explained all this in detail, when reading my paper (Kammerer, 1923 a) before the Linnean Society of London in May, 1923, Professor Goodrich, in the course of the discussion, again voiced the very objection I had taken special pains to refute.

Who, in consideration of all this, can fail to remember the experience encountered over and again with the fair sex who, after long hours of explanation, still offer the rejoinder: "Why, just because!"

If we accept as "hereditary" only that which is unchangeable, then, of course, we have not only excluded right from the beginning every inheritance of changes, but have again disposed of this question once and for all (Chapter XIII). If changeable characteristics are not permitted to be hereditary, and if hereditary characteristics are not to be changeable, then, just as in the dark Middle Ages, it is left to us only to expound the fixity of species. And with this, not only the inheritance of acquired characteristics, but the whole theory of evolution is dogmatically done away with.

But because the shortsightedness of my opponents prevented me from raising a second generation of my "seeing Proteus," which most probably would have proved the inheritance of the acquired faculty of vision, I am going to choose from my experiments another example (Kammerer, 1910 d) where the stage of development of an organ was also hereditarily intensified—an example which extends over more than one generation.

CHAPTER XXXII

PROGRESSION AND RETROGRESSION

THE South European Wall Lizard, *Lacerta serpa,* living at large, or kept at an intermediate temperature (68-86° F.), is green on its back with three brown lateral stripes, or string-like arranged spots. Subjected day and night to a temperature of about 98 to 104° F., within the course of eighteen to twenty-four months, the lizard becomes pitch black. Since these experiments required high temperature, it was possible to conduct them in my native country only before 1914, because "black diamonds" later on became just as expensive as the white kind that are ordinarily used for baubles. From eggs deposited by blackened lizards at lower temperature, almost normally colored young emerged. This nearly deceived me to the point of assuming the non-inheritance of the artificial blackening. But later on, the young changed to black, though now living in an intermediate temperature in which specimens of the same species remained green with brown stripes (Kammerer, 1910 d).

I am not drawing upon this example on account of the color changes, because these are too similar to the color changes on butterflies (Chapter IV), beetles (Chapter VII), and salamanders (Chapter XVI) to offer anything essentially new. My reason for using this example is, however, to show that the experi-

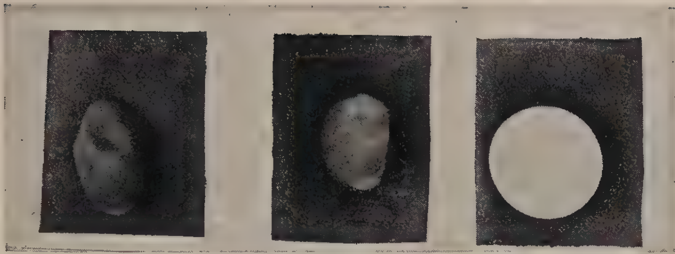


FIG. 40.—WALL LIZARD (*Lacerta serpa*)

: *A*, Normal; *B*, from the first batch, when kept at a temperature of 98-104° F.; *C*, from he second batch of the same female, again at a temperature of 98-104° F.—(Original)

FIG. 41.—MOUNTAIN LIZARD (*Lacerta vivipara*)

he top, three mature specimens. *A*, male; *B*, female; *C*, female darkened by being kept at mperature of 77-86° F. This female is pregnant and a product of the egg-depositing breed not from the breed bearing living young. *D*, Normally new-born young. At the bottom, three eggs without shells (the blackish embryo only covered by the egg-membrane). , *G*, From the first batch raised at 77-86° F. *H*, An egg with an onion-skinlike shell from second batch of the same female raised at an identical temperature. *I*, Offspring of such

menter is almost never able to accomplish only one desired change on his object of experimentation, without other unintended changes appearing simultaneously.

It is on account of one of the latter unintended changes that I am offering this example to illustrate progressive adaptability. The wall lizard, under normal conditions, deposits elongated eggs which are covered with an onion-like skin and are quite soft, because they are poor in calcium. For this reason (as shown in Fig. 40, at the left) the eggs often show indentations after having been touched by the hand of man. Now it is to be noted that the first eggs deposited while the lizard was kept at a higher temperature had a thicker shell and were no longer so elongated (Fig. 40, center). Finally, the lizard blackened as the result of being kept in high temperature, deposited hard-shell eggs, rich in calcium, which at the same time had become perfectly round (Fig. 40, right). A family of nocturnal lizards, the Geckoes (*Gecconidæ*), which have flattened toes and are to be found in warm climes, deposit similar round eggs which, aside from being smaller, also resemble greatly the eggs of a land tortoise (*Testudo*).

Young lizards emerging from the hard-shell eggs again deposited hard-shell eggs, even if kept at an intermediate temperature as specimens kept there for controlling purposes, with the latter depositing soft-shell eggs. Since the lizard is a "cold-blooded" (poikilothermal) animal, only in the beginning of the experiment was the temperatural stimulation necessary which penetrates all strata of the body, thus stimulating the calcium-secreting glands of the oviduct to

intensified action; and that, in turn, resulted in the egg shells becoming thicker and harder. Later on, this intensified secretion of calcium became a "habit," if I may say so, so that the temperatural stimulation could be abandoned on parents as well as on children.

By the temperatural variation as described here, another very practical adaptation was achieved: the hard-shelled egg is much better protected against drying up in high temperature. This shows that practical, enduring characteristics are effected by direct adaptation, without selection playing the part of a sieve.

The up and down, the back and forth, of the change of species inducted and conducted by the inheritance of acquired characteristics is, as already shown in the case of the Midwife Toad (Chapter IX), noticeable not only on living beings as decidedly different as the *Daphnia* and the lizard, but also on closely related species, even on specimens within the realm of one and the same species, showing that development may just as well be progressive as retrogressive. In the propagation of a lizard with a southern habitat, if the rising of temperature resulted in a progression, an improvement; then in the lizard with a northern habitat and living in mountainous regions, a rise in temperature affecting its propagation would amount to a retrogression, thus placing the lizard at a disadvantage.

The Mountain-lizard (*Lacerta vivipara,* Fig. 41 *a, b, c*) in its wild state bears living young (*d*); but the moderate rise of temperature from 77° to 86° F. suffices to change the lizard to an egg-depositing animal (Kammerer, 1910 d). The first of these eggs

(*e, f, g*) have no shells and appear to be dark, because the dusky embryo (*i*) is visible through the transparent egg membrane. Such eggs, even under normal conditions, are sometimes deposited with this difference: the membrane covering the egg is burst by the young during the first few minutes or, at the latest, after a few hours, while in high temperature they require a maturing period extending over many days. At the second laying period, the same female in a raised temperature produces eggs which are covered with a non-transparent, onion-skin-like, yellow-white shell (Fig. 41 *h*) similar to those laid by other lizards, originally also by the *Lacerta serpa,* which was drawn upon before as an example. The young of the *Lacerta vivipara* which, for the duration of weeks outside of the mother's womb, remained in the egg to await the full maturity of the egg, again developed into egg-laying specimens, even if kept at a lower temperature where specimens kept for controlling test purposes bear living young.

CHAPTER XXXIII

ACQUISITIONS BY THE PSYCHE AND THEIR INHERITANCE

UP to now, all the proof for the inheritance of acquired characteristics pertained to physical characteristics alone, with the exception of the Midwife Toad (Chapter IX), which, as soon as it feels the urge of mating, makes for the water instead of remaining on land. But I do not want to conclude the biological part of this book without having shown, on a broader basis, that mental characteristics also are subject with regularity to the same law; that not only physical characteristics, but psychic as well—habits and urges—may be acquired and passed on.

An example illustrating this point, which most probably will stand broadest generalization, pertains to the nest-building instinct of the moth *Gracilaria stigmatella* (Fig. 42). The caterpillars of these small butterflies (*a*) build their nest with a willow leaf, upon which they feast, wrapping the point of the leaf around themselves and fastening it securely with the strand which they secrete (*A*). Christian Schroeder (1913 b) made it impossible for them to follow this habit, by cutting off all the points of the leaves of a potted willow plant. But this did not deter a great number of the little caterpillars; for finding the point of the leaves missing, they took recourse to one (*B*)

or the two (*C*) edges. The cutting off of the point of the leaves was repeated, during the growing up of a second generation of caterpillars, with a similar result. From these adaptable little caterpillars, Christian Schroeder bred the mature moths which were offered a chance to leave their eggs on another willow bush

FIG. 42.—MOTH *Gracilaria stigmatella.*

At the top, right: mature moth—*a,* caterpillar; *A,* willow leaf, curled in from the point down, wrapped around the caterpillar. *B,* The caterpillar (on account of the point of the leaf being cut off) wraps itself in with the edge of the leaf. *C,* both edges rolled in. *D,* In spite of the point of the leaf not being cut *off,* the grandchildren generation wraps itself up with the edges

(Chr. Schröder, 1903 b)

whose leaves were left untouched. Quite undisturbed, these eggs matured into a third generation of caterpillars; and, even though the nest-building instinct of the young caterpillars was not in the least encroached upon, a number of them took recourse to the same

method of wrapping themselves with the edges of the leaves (*D*).

The speed with which old established instincts can be changed—first under the pressure of exterior influences and, later on, even without this pressure—is especially surprising in the case of *Gracilaria stigmatella.* It would surely have been fitting to repeat Schroeder's ingenious experiment as often as possible instead of ignoring it, as was consequently done with only a few exceptions, in order to dispose of the inheritance of acquired characteristics without even testing the theory.

However, it is not feasible to becloud truth everlastingly. At the International Congress of Physiologists at Edinburgh in 1923, a paper was read which, to those who were satisfied that the inheritance of acquired characteristics, and especially the inheritance of mental characteristics, had been disposed of once for all, must have sounded even stranger than the experiments which Schroeder conducted on the moth. At this congress, Pavlov reported on training experiments with mice, which in the sixth generation resulted in striking proofs of heredity. At the sound of an electric bell, a mouse was released from its cage to receive a little piece of cheese which, up to that time, had been kept beyond the sphere of the olfactory perception of the animal. This experiment was repeated at short intervals until, at the three-hundredth time, the mouse acted independently, sniffing around excitedly with a simultaneously intensified function of the salivary glands, trying to detect the cheese which now was not in proximity. To express it more colloquially, the mouse's mouth "watered."

In the progeny of the mouse "trained on cheese," the time for the associative mental connection between the sound of the bell and what it stood for—the leaving of the cage and the presence of a delicacy—gradually grew shorter; in the second generation, a hundred, in the third, only fifty "lessons" were necessary. In the last generation of which Pavlov reported at Edinburgh, only five signals were necessary to release the characteristic searching and intensified function of the saliva glands, which in the great-great-great-grandfather had taken sixty times as long to be inducted. "I think it very probable," declared Pavlov in a lecture held in July, 1923, before the Battle Creek Medical staff, "that after some time a new generation of mice will run to the feeding place on hearing the bell, with *no* previous lesson."

In the light of exact experiments, numerous singular observations which carry no evidential value when considered singly, gain in intensified meaning when considered collectively.

A transformation of the head louse (*Pediculus capitis*) into the body louse (*P. corporis*) is related by Howlett. He and Patel reared head lice on their bodies. In the first generation the lice showed a tendency to migrate to the head. But this character was modified in the offspring some of whom showed no tendency to migrate headwards. The eggs of these offspring were mostly laid on clothing. In the third generation the migrating tendency had largely disappeared. Chitinization and color were also greatly modified. If these lice had been sent without explanation to an expert more than seventy-five per cent

of them would—according to Howlett—have been pronounced body lice.

C. M. v. Unruh reports about a homeless Scotch greyhound which had made Central Park, of New York City, his playground and was fed by sympathetic night policemen. During the day, this dog hid in the bushes and only went down to one of the lakes to drink. One day, the vagrant dog succeeded in establishing relations with a pampered French poodle (female) and one of the male offspring of this affinity, when still young, was taken to Germany. Even at the age of fifteen and a half years, this offspring of the vagrant dog of Central Park could not suppress his desire to roam, so as soon as night came, he restlessly paced up and down in the yard in which he occasionally was shut in. During the day, he was only thirsty, never hungry. He refused food, even delicacies, but as soon as night came, he eagerly swallowed everything that was offered to him, and he preferred to eat out of the hand than out of a bowl. All these characteristics which his homeless father had acquired were resurrected in his pampered son.

Under date of March 1, 1920, Mr. Arthur Schuetz, M. E., reported to me: "For about two weeks I have owned a five-months-old pedigreed Dobermann male dog, named Rolf. The animal is absolutely untrained. For the time being, I am only trying to make him obey me. Last night I took a walk with him. To test his power of perception, I asked a friend standing at the street corner to walk up to me, then to stop when only five steps away and make a motion as if he were going to strike me. Rolf watched the whole procedure apparently quite uninterested, and

quietly permitted the friend to approach me. Meanwhile I kept the dog on a short leash. At the very moment when the friend lifted his hand, Rolf's hair bristled and, barking viciously, he jumped at the man. I pulled him back so hard that he tumbled over, but he immediately got up and again made for my friend. Only after my friend had dropped his hand and did not move, the dog quieted down. To me, who have trained dogs 'on the man,' the most interesting thing was the *way* in which the dog acted. It was exactly the same as is usually observed in highly trained police dogs when attacking criminals—a distinct behavior which is well known to everybody who comes in contact with police dogs."

Up to now it was easy enough for the opponents to dispose of experiences of the type just described. They would simply have been grandly waved away with the remark that defending its master is nothing but a general characteristic of the domesticated dog; training for such ends would be superfluous.

And whoever would insist on the peculiar behavior of the dog, which seems so essential to the connoisseur, would be told that man chooses his animals for their useful characteristics, detected previous to their domestication. Man did not develop these characteristics as an afterthought but, at the most, only succeeded in intensifying them by training and breeding. The tendencies for such development were always there. Even the police dog was not developed by man, but only perfected by training and breeding in pure strain. The race as such always possessed those characteristics which made these dogs suitable for "police service." It was these inborn tendencies, and not the inheritance

of training results, that manifested themselves in the puppy Rolf.

The case reported by v. Unruh could be dismissed with a grand gesture and the remark that almost all, but especially pampered, dogs prefer to eat out of the hand of the master rather than out of the bowl. The tendency towards nocturnal roaming and nocturnal feeding is also nothing remarkable—just an atavism resurrected from the remote past of the dog. These characteristics, common with beasts of prey, were not sufficient reasons for tracing them back to the life which the homeless dog led in Central Park.

Arguing along these lines becomes more difficult in cases where the releasing motif could not have prevailed as long as the animal in question was not domesticated. The most remarkable abilities of hunting dogs, which today are often found to be already inbred, are ascribed to some preëxistence right from the beginning of time. The necessity, even the mere possibility of a later "acquisition" is stubbornly denied. But the meaning of the report of a gun can only have become a part of the hereditary memory *("Mneme")* of the hunting dog since the discovery of powder and ball. On account of this, the very careful, critical physiologist, S. Exner, relates how a young hunting dog, never before employed at a chase, as soon as he heard the first report of a gun started to search for a partridge that was not hit at all and which, for this reason, the dog could not very well have seen tumbling down to earth. This observation is partly responsible for Exner's having become convinced of the inheritance of acquired characteristics.

However—and this is the most weighty, but also

almost the only objection which may be raised against such observations with any substantiation—within the realm of natural sciences, there is no convincing proof but the experiment! From this, it follows that there is no persuasive proof for the inheritance of acquired characeristics, with the exception of the breeding experiment. Exner's observation on the hunting dog and similar observations, however, appear scientifically important when considered together with the experiments conducted by Pavlov on mice, in which the ringing of a bell played the same part as did the report of a gun in the case of Exner's hunting dog.

In the same manner, well-known manifestations in the realm of human mentality assume new importance and become enlightening only when we are able to compare them with experimentally tested manifestations on animals, *i.e.,* by drawing upon the analogy of the observations. Such important questions as the origin and the systematic development of talents and genius (see A. Hock) depend on them—questions which up to now have remained the plaything of the most unbelievable opinions and superstitions and which, only by way of analogous argumentation, may be solved on a strictly scientific basis.

Already in Chapter III of this book, the opinion was voiced and substantiated that experimental proof on plants and animals may be accepted as valid, since it is impossible to effect an equally convincing experimental proof on man. In two examples, I wish to illustrate how every undertaking to explain phenomena of heredity on man, by studying man himself, is doomed to utter failure right from the beginning.

It is frequently assumed that Wolfgang Amadeus

Mozart inherited his musical talent from his father, Leopold Mozart, which seems quite probable (Kammerer, 1912 d). But Wolfgang Amadeus from earliest childhood lived in an atmosphere simply imbued with music. Who, now, will determine how much of Mozart's musical talent was inborn, and what was acquired by him through assimilating what his youthful ear and brain acquired subconsciously? To decide, even to a certain degree only, what was inherited and what was acquired, it would have been necessary to remove the newly born infant from the parental home and keep him away from each and every musical influence. Only then, when, in spite of this removal from a musical atmosphere, the urge towards things musical (which is frequently only the result of paternal promptings) would have broken through spontaneously, one would have been justified in assuming with some certainty that here was a case of inheritance. But such an experiment would have been quite risky, even reckless, and would have had, possibly, to be paid for with the loss of many of the finest pieces of music ever written.

More about the relations between talents and education, and talents and inheritance, will be said in Chapter LII, in the "Eugenical Part" of this book. Right here it is the intention merely to touch upon, and not solve, the problem and throw light on the intense difficulties which the solution of this question faces.

CHAPTER XXXIV

THE INHERITANCE OF DISEASES AND IMMUNITY

QUITE similar difficulties arise in reference to the so-called inheritance of diseases (Orschansky). Are we dealing here with an inherited disposition or with a newly acquired disease, brought about through living in a diseased environment? This is a question which, as a rule, cannot be answered satisfactorily.

The same uncertainty prevails regarding the contrary of the inheritance of diseases: *i.e.*, immunity—the inborn resistance against certain diseases. Is this immunity actually transmitted by the human egg and the human sperm cell to the progeny and so, in this way, inherited? Or does the embryo acquire this immunity only later on, either by absorbing this characteristic from the placenta or by being lactated with the mother's milk? Which, of course, would exclude these phenomena from the conception of inheritance and class them as repeated new acquisitions. Exactly the same question extends to alcoholism, assumedly hereditable, and its consequences. But in reference to immunity, we again step on *terra firma*, because, within the scope of these questions, we may draw upon significant experiments on plants and animals. I only wish to mention a few of the most dependable of them:

Gley and Charrin immunized rabbits against the

toxin of the *Bacillus pyocyaneus* whose virulence usually is deadly poison to these animals, even if administered in ever so small doses. Progeny were bred from rabbits of the following mating combinations: Immune mother with immune father; immune mother with non-immunized father; immune father with non-immunized mother. The progeny of the first combination were, regarding immunity, the most thoroughly immune, while the third combination yielded the least immune specimens. But just the same, the progeny was even then, for a certain length of time, immune when only the father had been immunized. But as the latter has no other means of transmission than his sperm cells, one is justified to a certain extent to speak about the inheritance of immunity.

At the same time, with respect to the immunity of the mother, it becomes manifest there actually exist other means of transmission. O. Hertwig (1909) is of the opinion that the anti-toxin generated in the maternal body is able to collect much quicker in the yolk of the eggs than to penetrate into the egg-nucleus. Moreover, in considering the immunity of the mother, placenta and lactation have to be taken into consideration.

CHAPTER XXXV

THE INHERITANCE OF ALCOHOLISM

SIMILAR questions to the foregoing block the way, regarding the inheritance or non-inheritance of alcoholism, or other vices originating from the habitual consummation of poisonous drugs. By inheritance, it is understood that there is a specific tendency of the germ plasm, which results in the reappearance of a characteristic of a forebear in the progeny. To be sure, we cannot speak about inheritance if the father is subject to *delirium tremens* and the son, on account of this, suffers from other mental disturbances; or if the mother, on account of habitual imbibing, suffers from a shrinking of the liver, or kidneys, or suffers from a fat heart, and the children for this reason are partly subject to tuberculosis, partly epileptic, or both. These are unspecific after-effects which were brought about by a poisoning of the germ plasm (see Forel's *Blastophthorie*) and not by a certain germ-plasmic determinant.

Even quite a number of experiments on animals do not reach farther than this observation, which hardly touches the problem of inheritance and which only proves that alcohol tends to wipe out the power to live and to propagate, without strictly limited and tangible characteristics having been acquired or passed on. Within this scope belongs the experiment of P.

Schroeder (1913), who succeeded in developing chronic alcoholism in rabbits. These rabbits propagated themselves through six generations which suffered from diseases, insufficient development of the young, and a diminishing of propagative power.

Another experiment of this sort is the one which Stockard and Craig conducted on guinea pigs, which they caused to inhale alcohol vapors without, however, intoxicating them. The result was that out of fifty-five matings, only forty-two developed into normal pregnancy. Of eighteen fully developed young, born alive, only seven (among which there were five extremely undersized specimens) lived longer than just a few weeks. In contrast to these fifty-five matings of slightly alcoholized parents, nine matings of controlling specimens resulted in seventeen young, all of which remained alive and developed into big, healthy specimens. All these experiments proved that alcohol is highly poisonous to the germ plasm and results are transmitted to the later generations, as unspecific manifestations of degeneration. But the experiments do not show even one genuine and certain manifestation of inheritance.

I know of only one experiment which, in this respect, proves what alcoholism is liable to bring about—an experiment which throws light over all those other experiments and manifestations of alcoholism on man, which, up to now, has remained in obscurity. This experiment need only be performed more frequently and in different combinations, especially on other animals than mammals, or with the mother abstinent and only the father alcoholized.

Kabrhel took two dogs, male and female, and after weaning them, accustomed them to drink beer. He

succeeded with great difficulty, having to mix the beer with salted food and depriving the dogs of water, in order to get them habituated to the consumption of alcohol. The dogs continued to consume alcohol even around the time of mating, and during the gravidity of the female. The latter bore four puppies which were apparently normally nursed, and they developed normally. When the mother's milk was supplemented by solid food, they also sought the beer presented to the mother and refused to drink water. Of course, here the objection may be raised that the puppies were not kept in seclusion but simply could follow the example set by their alcoholized parents.

The result is not without a certain comic element, but of course, translated into the all-too-human, it becomes tragicomic. It demonstrates that the pernicious urge is a hereditary characteristic and that, even if originally foreign to the animal in question, it had been planted in him with arduous training. Parents and tutors who influence their children and wards to the use of alcohol are criminals; but, for the most part, they are ignorant of the formidable responsibility of their actions.

Liquor attacks the germ cells directly; and verily it may be termed "germ toxin." Nicotine, on the other hand, as far as is known, has an especially poisonous effect on the nervous system, and is a nerve toxin. As far as I know, experiments regarding the influence which this poison exerts on the germ indirectly have not as yet been conducted. An observation made on dogs was related to me by a friend, and I am repeating it here for what it may be worth: Three successive generations of Pomeranians, right from their earliest

stages of development, showed a decided inclination to chew tobacco. The taste for tobacco, manifesting itself spontaneously in the dog, is something so unnatural that it could not have been brought about by anything different than an environment (in this case, a men's club), in which quantities of tobacco were easily accessible to them.

CHAPTER XXXVI

THE INHERITANCE OF ACQUIRED CHARACTERISTICS IN MAN

ALL this tends to show that for some time already our discourse moves in the exalted sphere of man. Only experimental proof have we admitted as infallible. On account of analogies drawn from the animal kingdom, this proof would become applicable to man even if we were unable to furnish any other symptom of the inheritance of acquired characteristics in man. But we can do that! We have indirect and incomplete proofs, symptoms and signs, which, within the realm of human nature, speak for the inheritance of acquired characteristics and which are the more weighty as they are in complete accord with the experimental research work on living beings below the species *homo sapiens*.

An enormous amount of such positive instances pointing to the inheritance of acquired characteristics in man and which offer themselves in the so-called *"rudimentary organs,"* Plate (1913) values even as unassailable evidence. Wiedersheim counts more than ninety of such remnants of once well developed and well functioning organs, which are to be found in the human body. Best known of them is the appendix. Another one is the crescent-shaped fold of the inner corner of the eye, which is the remnant of a third lid,

and is still to be observed in birds and reptiles as the nictitating membrane. Still other remnants are the muscles which were necessary to wiggle the ears. There are still specimens of the species *homo sapiens* living in our time, who are able to wiggle their ears even if they cannot move them in all directions—a faculty which is absolutely indispensable for animals living in herds and at large, as this enables them to sense approaching danger.

Once in a while, *atavisms* may be observed which bring out the one or the other forgotten and retrogressed faculty more strongly; like surplus nipples, reminiscent of the time when our animalistic forebears were more fertile and the mother's breast could nourish more than two offspring at once. Other atavisms are the lanuginous covering of the embryo and the development of the spine, tending to form a tail whose remnants constitute the degenerated caudal vertebræ, also called coccygeal vertebræ, which normally does not show in soft parts—all these are reminders of the animalistic epoch of our forebears in the dark past.

All these rudiments, be they atavistic or of a recent date, are proof of formerly more distinctly developed forms which, by diminished or modified usage (sometimes by not being used at all), must, by necessity, have degenerated in the course of time. Because the modified, mostly diminished, or even entirely checked, use of the one or the other organ resulted from changed circumstances of life and had their roots in changed necessities of life, these rudiments are evidence of just as many hereditarily acquired characteristics whose heredity is proved by a process of gradual and local degeneration.

CHAPTER XXXVII

INHERITANCE OF THE CALLOSITIES OF THE HUMAN SOLE

To the old anatomist, Albinus (whose work was improved, in the course of time, by the perfection of the microscope), and to the only too early deceased contemporaneous zoölogist, Richard Semon (1913), we are indebted for the discovery of an hereditary phenomenon of adaptability not belonging to a long vanished past, but one which can be observed even today and in every new generation of man. Here is a case of the heredity of acquired characteristics which, even though it is observed in man, carries almost as much conviction as a methodical breeding experiment.

On the sole of every human being (Fig 43) there develops callous skin, which is the more callous the older and heavier the person is. People who walk a great deal are more subject to this callous and, of course, it develops much more strongly in people who go barefooted than in those who wear soft leather shoes. This callous pad is most strongly developed along the so-called Meyer's line (*a-b*), that is, on those spots where a normally built foot touches ground most frequently—the heel, the ball of the foot, and under the first digit of the big toe. Being bedridden for any length of time, the horny pad retrogresses. All this goes to show most convincingly that the cal-

lous spots on our soles are brought about by the weight of our body. For this reason, the horny pad is an acquired characteristic, a proof of functional adaptability.

FIG. 43.—HUMAN SOLE

Showing the so-called "Meyer's line" (*a-b*) where pressure develops callosities. Left, top: microtome section through the skin of the sole; below, section through the dorsum of the foot, both from a human embryo of seven and one-half months. *HSch*, horny layer; *KSch*, germinal layer; *PK*, papillary body

(R. Semon, 1913)

Scrutinizing with the naked eye the sole of a newly born child, we are unable to observe the slightest trace of a horny pad; the sole of an infant is as smooth and tender as the petals of a rose.

Well, it seems there is nothing to the heredity of acquired characteristics, after all. But what a surprise if we carry on our investigations through a microscope! (Fig. 43, left). Even in an embryo of seven months (long before a child makes its first steps) we notice on the skin of its sole unequivocal symptoms of

an accelerated growth—the horny layer of the epidermis of the sole. And, again, those regions which will later on have to sustain the most weight are thicker than those which cover the dorsum of the foot. But the germinal layer and the papillary body beneath it, which is already a part of the true skin, forms folds on the sole—folds which always signify lively tissue growth where, in spite of a new growth, a certain space must suffice. On the dorsum of the foot, the papillary body and the germinal layer do not form folds, but cover the foot in an undulatory manner, because here no essential increase of size and thickness occurs.

A genuine horny pad cannot be observed either on the sole of the embryo or on the sole of a newly born infant, but there are unmistakable beginnings of such a horny pad which develop promptly as soon as they are needed. The reader may deduce from this fact a conclusion which, in regard to the enormous number of cases of adaptability in man, is of utmost importance: that the adapted characteristic is not passed on in its ultimate form. But do not be deceived by this to deny heredity altogether, because assuredly a corresponding disposition, a corresponding tendency, is passed on and, together with this, the necessity to fully develop these tendencies as soon as circumstances in life arise, which make it necessary or which are favorable for such a development.

CHAPTER XXXVIII

SUMMARY: FACTS IN FAVOR OF THE INHERITANCE OF ACQUIRED CHARACTERISTICS

Organic and mental characteristics are to be divided into two distinct groups: Such characteristics as have been passed on to the present generation by their forefathers, and those which have been acquired by the individual in the course of a lifetime. We plainly have to divide physical and psychical characteristics into inherited and acquired characteristics.

Regarding the inborn characteristics—as far as they manifest themselves in a race, a nation, or a family—there has never been any doubt about their being passed on to the next generation, just as they had been passed on from some former generation.

But the important question that looms up before us is: Can characteristics, individually acquired—personal characteristics in the strictest sense of the word—be passed on to posterity?

Let the facts speak for themselves.

A

Direct or Experimental Evidence

The following artificially induced changes were transmitted to progeny of directly changed progenitors:

1. *Locomotion:* The sniffing around of those mice which were trained by Pavlov to expect a delicacy as soon as they heard the ringing of a bell. On larvæ of the European Willow-beetle (*Phratora vitellinæ*) Christian Schroeder observed (1903 b) that, with downy-haired willow leaves as a home, they assumed a "mining mode of living," *i.e.,* they drilled holes into the texture of the leaf, instead of just working along the surface.

2. *Nourishment:* In the course of the last mentioned experiments, an additional number of beetles with every new generation voluntarily emigrated to the downy-haired leaves for their nourishment, instead of feasting on the smooth leaves they originally inhabited. For the depositing of eggs, they also chose the new abode.—Caterpillars of a number of butterfly species (for example, of the *Ocneria* [*Lymantria*] *dispar*), in the course of experiments conducted by Pictet (1905), were forced to change from the plant on which they were accustomed to feed. The new food plant resulted in color changes in the mature butterflies which proved to be hereditary.—The training of the progenitors to imbibe beer freely, as in the experiment of Kabrhel on dogs, resulted in the progeny taking to alcohol spontaneously.

3. *Nesting Instinct:* The experiments of Christian Schroeder (1903 b) on the moth *Gracilaria stigmatella,* to wrap the edges of the leaves around themselves instead of the points.

4. *Propagation:* The depositing of hard-shelled eggs instead of soft-shelled ones (lizard *Lacerta serpa;* Kammerer, 1910 d).—The depositing of eggs in water instead of on land (tree-frog, *Hyla arborea;*

Kammerer, 1907 b).—The depositing of eggs instead of bearing living young (lizard *Lacerta vivipara;* Kammerer, 1910 d, and spotted salamander, *Salamandra maculosa;* Kammerer, 1907 c). The development into larvæ with gills instead of being born as a fully developed young specimen (black salamander, *Salamandra atra;* Kammerer, 1907 c). The giving birth to fully developed young, breathing through lungs instead of gills (spotted salamander, *Salamandra maculosa;* Kammerer, 1907 a, c).

5. *Development:* Modified in all of the enumerated examples of hereditarily changed methods of propagation. The passing of several stages of development within the egg as in the case of the Midwife Toad when bred on land; and the premature emerging from the egg in the case of the same animal when bred in water (Kammerer, 1909, 1910 e, 1911 b, 1914 b, 1919).—Also the development of three sets of gills instead of a single pair, and retrogression of the yolk sac, because the latter has become unnecessary. Development of longer and more pliable gills, poorer in pigment, richer in blood vessels, together with premature development of pulmonary alveoli of the lung in all those cases where the Midwife Toad and the salamander became more independent of the water while still in their embryonal or larval stage.—Development of shorter and stiffer gills, poorer in blood but richer in pigment, adjusted to the body at an angle instead of lying flat, retarded development of pulmonary alveoli of the lung, wherever the development became more dependent on the water. Simultaneously with an intensified independence of the water, retrogression of the margin and the muscles of the tail.—Retaining of

immature forms (sexually mature tadpoles of the Midwife Toad, *Alytes obstetricans;* Kammerer, 1909).—Progression towards final stages, only rarely reached at large, together with an hereditary inclination to metamorphose into these final stages. *(Axolotl, Amblystoma mexicanum;* Marie von Chauvin).

6. *Periodicity:* Early maturing of maize effected by mutilation and twisting of the stalk (Blaringhem, 1907).—Time of blossoming climatically acquired and hereditarily retained (for example, in the Golden-rod, *Solidago virgaurea;* H. Hoffmann, Detmer).—Hereditary change from annual plant to perennial plant, effecting a prolongation of life (for example, *Ricinus communis;* R. Wettstein).—Some plants (*Acacia* and *Mimosa*), within a period of approximately twelve hours, go through a certain rhythm of "sleep movements." During the day, they unfold their leaves and fold them when night comes. Semon (1905, 1908) raised young plants under conditions deviating from the normal change from light to darkness. He sometimes gave his experimental specimens as little as six hours for the rhythmical change, and sometimes he gave them fully twenty-four hours. He succeeded in making the leaves respond accordingly, so that when the latter, having acquired a certain rhythmical habit of unfolding and folding, were placed either into permanent light or permanent darkness, they gradually returned to the inborn twelve-hour rhythm, even though only their parents, but never they themselves, had been subject to the natural rhythm. These experiences of Semon were doubted for quite some time, but eventually were fully corroborated by Pfeffer. The change from day to night leaves impressions on a plant

which become hereditary.—Annual periods, that is, results from changes of the season, also proved to be hereditary in experiments conducted by Semon (1921). The peach tree, in its native habitat, bears foliage only during the summer, but if transplanted to a tropical clime, it becomes hereditarily verdant (experiments by Bordage). Hereditary deviation from the change of generations, that is, from generations propagating themselves monosexually to bisexually, became apparent in experiments on the water-flea, *Daphnia* (Woltereck).

7. *Forms:* Increased percentage of the salt contained in the water resulted, after a few generations, in a partial loss of the bristles of the fresh-water tube-making worm, *Tubifex,* which usually inhabits fresh-water ponds. The progeny became used to salt water and were unable to stay alive in fresh water (Ferronière).—Decreased percentage of salt in the crustacean *Artemia salina* results in deeply cut, long and richly fringed forked tails which, from generation to generation, ever tend to intensify their similarity to the "Fairy-shrimp" *Branchipus,* living in fresh water (Schmankewitsch; after many years of skepticism and of alleged errancy, only lately fully corroborated by Russian scientists).—Increased temperature of the water changed low helmets into high ones; the short abdominal sting adjusted at a sharp angle straightened out and developed into a long sting, in experiments with the water-flea *Daphnia longispina,* and became hereditary after the experimental specimens had been kept at a raised temperature for two years (Woltereck).—Development of the nuptial pad on the forearms of the male Midwife Toad, *Alytes obstetricans,* when bred

in water (Kammerer, 1909, 1919).—Development of over-long siphons, after repeated amputation and regeneration, in the sea-squirt, *Ciona intestinalis* (Kammerer, 1920 a, 1923 d).—Nine generations of rabbits with defective eyes, after this defect had been chemically effected in the first generation (Guyer and Smith).

8. *Color:*

a) *Under changed quantity of illumination* (light or dark): Hereditary atrophy of the color substance of the eyes in water-fleas (*Daphnia pulex*) kept in the dark (Kapterew).—Hereditary retention of colorlessness, probably acquired, or only poor amassing of pigment in the flatfish, *Pleuronectes,* in spite of being exposed to illumination from below (Cunningham, 1891, 1892, 1895).—Hereditary retention of an acquired amassing of pigment by keeping the newt Proteus in the light (Kammerer, 1912 b, 1920 d).

b) *Under changed quality of illumination* (color of environment): Hereditary color adaptations in salamanders, newts, toads, frogs, lizards, and snails (Kammerer, 1910 c, d; 1911 c; 1912 a; 1913 c).—Hereditary color adaptations in the larvæ of butterflies (Leonore Brecher, Duerken); in the stick-insect *Carausius* [*Dixippus*] *morosus* (MacBride and A. Jackson; Przibram and L. Brecher).

c) *Under changed conditions of humidity:* Multiplication of yellow spots of the spotted salamander (*Salamandra maculosa*) when subjected to intensified humidity; darkening of the yellow spots by reduced humidity (Kammerer, 1913 a, c).

—Also experiments of Tower on the Potato Beetle (*Leptinotarsa*); experiments of Pictet on butterflies, especially on *Ocneria* [*Lymantria*] *dispar.*

d) *Under changed temperature:* Raising of a discolored dwarfed race of the Colorado Potato Beetle (*Leptinotarsa decemlineata*) when exposed to heat (Tower).—Development of hereditarily blackened forms of the Nettle Moth (*Vanessa urticæ;* Standfuss) and in the Great Tiger Moth (*Arctia caja;* E. Fischer) by subjecting them to cold; in the Gooseberry Moth (*Abraxas grossulariata*) subjected to heat (Christian Schroeder, 1903 a).—Hereditary heat-melamism in lizards (Kammerer, 1910 a, b; 1913 f) and elimination of color differences between the sexes in lizards (Kammerer, 1910 b).

e) *Under changed food conditions:* Experiments on butterflies by Pictet, 1905.

9. *Size:* Climatically acquired differences as to size of trunk, leaves and blossoms, also color differentiations of all those parts of plants growing in mountainous regions are hereditarily retained by seedlings taken from them and cultivated at a lower altitude (experiments by Cieslar on pine, larch, and other coniferæ; by Zederbauer (1908) on Shepherd's Purse, *Capsella bursa pastoris*).—*Vice versa,* the hereditary intensification of alpine characteristics by transporting into higher altitudes (experiments by Weinzierl on grasses and with other plants by Bonnier and Lesage; see also what Kropotkin reports in this respect).—Dwarfed growth in the terrestrial forms, giant growth in the aquatic forms of the Midwife Toad (*Alytes;* Kammerer, 1909).—Dwarfed growth and reduced

growth of hair in rats kept at a higher temperature (Przibram, 1910 a, b); intensified growth of peripherous parts of the body, especially of the tail, most remarkable in rats kept at a raised temperature, which in regard to their body as a whole generally remain smaller (Przibram, 1910 b, 1923 b); analogous experiments on mice (Sumner).

10. *Disease:* Epilepsy, induced by operation, hereditarily retained by guinea pigs; also protruding eyes (*Exophthalmus*), etc. (Brown-Séquard).—Successful testing of the experiments of epilepsy induced by operation by Romanes (1892-97), Dupuy, Westphal, and Obersteiner. (Compare also facts not in favor of an inheritance of acquired characteristics, Chapter XXXIX, A, 6.)

11. *Immunity against poisons:* Hereditary immunity against the *Bacillus pyocyaneus* in rabbits (Gley and Charrin), even in cases of immunized fathers and non-immunized mothers which, in all other experiments regarding the inheritance of acquired immunity, proved unsuccessful. (Compare facts against inheritance of acquired characteristics, Chapter XXXIX, A, 7.)—Immunity intensified from generation to generation against wilt and rust in flax (H. S. Bolley).—Immunity of wheat against rust in crossing experiments, even with non-immunized plants, is in accordance with the Mendelian rules of heredity. The many experiments with bacteria and trypanosomes (hereditary intensification or reduction of disease inducing metabolism by becoming accustomed to it and change of hosts) are omitted here, because transmission by non-sexual propagation frequently is not considered genuine "heredity."

For the same reason, all experiments as to "heredity" with monocellular living beings (*protista*) have been omitted from this summary (see the writings of MacClendon, 1909; Jollos, 1921; Pringsheim, Semon, 1912; and Jennings, 1920, 1923). But, to be sure, they should not be quoted against the inheritance of acquired characteristics, as many authors are wont to do.

On the other hand, a number of examples not mentioned in the previous, more explicit chapters of this book were quoted within the summary.

Perusing the summary, it is easy enough to notice that quite a number of examples which are only mentioned here under one of the different classifications could be classified under more than one heading. Among these are the changes which plants of mountainous regions undergo in the lowlands. Within the scope of the summary, they are classified only under "Size," whereas they could also have been mentioned under "Form" and "Color." Others are the changes of sizes in rats and mice which were quoted only under "Size" but which could also be mentioned under "Form," etc.

B

Indirect or Non-experimental Proofs

1. *Immunity:* Continuing right from the last mentioned group of facts, in reference to the "direct proofs": the natural immunity of hedgehogs, polecats, ichneumons, Harrier-eagles (*Circaëtus gallicus*), etc., against the bites of poisonous snakes may be the result of these animals acquiring hereditary im-

munity by continuously battling with their poisonous prey. Regarding the hedgehog, it is well known that, in regions where there are no poisonous snakes, it is not at all immune. Snakes (*Ophibolus*) which predominantly live on other snakes are immune only to the poisons of those snakes which they feast on regularly.

Hereditary immunity is apparently the reason for the adaptation of parasites to those of their hosts whose bodies produce anti-toxins. These have to be combated first by the parasites and, therefore, certain species of parasites will thrive only on certain hosts, and every species of host gradually influences its parasites. It takes quite an extended process of readaptation, if the relations between the parasite and the host are to be changed. For this reason, the Common Mistle, for example, to be observed on hemlocks, cannot very easily be transplanted to poplars or fruit trees, and *vice versa*.

A reciprocal condition of this kind Klebahn observed in the fungus *Puccinea smilacearum,* usually to be found on all asparagus plants (*Smilaceæ*). Klebahn, for the duration of ten years, cultivated this parasite on the David's Harp, *Polygonatum multiflorum.* After this time, it was very difficult for the parasite to infect plants related to the host species, and it was especially difficult for the parasite to keep on developing if attached to such species as the Common lily of the valley (*Convallaria majalis*), the Wild lily of the valley (*Majanthemum bifolium*) and the One-berry (*Paris quadrifolia*). The same process experimentally effected by Klebahn most probably parallels those cases mentioned before where the same process

was effected naturally, manifesting the adaptation of the parasite's metabolism to that of a certain host. Wherever we observe a parasite (as in the mistle) adapted to a certain host, we may draw the aforementioned conclusion.

2. *Parasitism and symbiosis:* Mutual adaptations of organisms, living in close proximity, are not limited to developing resistance against the poison of a foreign metabolism and foreign albumin. These adaptations, as a matter of fact, embrace the whole appearance and all the functions of the organisms. Forms and habits of life of such organisms become adapted to each other, independent from the fact that such a union is advantageous to only one of the partners ("Parasitism") or both of the partners ("symbiosis").

There is, for example, the unique form of the *Adamsia* which does not resemble the sea-anemones at all and which, for mutual benefit, lives in a state of symbiosis with the Hermit crab (*Pagurian*). The flat body of *Adamsia* has two extensions with which it enwraps the shell inhabited by the little crab.—In Chapter XXIII, attention was directed to the characteristic manner in which polyps and *algæ* act toward each other with the *alga* proliferating in the body of the polyp. In Chapter XXVI, the symbiosis of ants with a number of plants was mentioned, with the plants, on account of the more or less close coöperation with the ants, developing very striking adaptations. All these adaptations, which doubtlessly have become hereditary, had to be required once, because the partners of any parasitic or symbiotic coöperation had to encounter each other at one time. It is not possible that they lived together from the very beginning of

time and were always endowed with these reciprocal characteristics.

With his "Principle of Virtual Rearrangement," Jackmann assiduously endeavors to prove that the legions of bacteria and other microbes permanently infesting the human body were an essential contributory cause of the species *homo sapiens* being divided into a number of distinctly different races.

3. *Habits of life:* The New Zealand Kea (*Nestor*), since raising of sheep became one of the biggest industries of Australia, from being herbivorous changed into a carnivorous bird. The African Ox-pecker (*Buphaga*), assisted by its pointed beak, originally plucked out the maggots only from the pasturing herds, but since European domesticated animals were imported to Africa, the ox-pecker resigned its symbiotic "policy" and, in many places of the African continent, changed to a parasitical one. Now, the ox-pecker eats the meat of the European domesticated animals and sucks the blood out of the wounds which he makes with his pointed beak.—The common sparrow in Europe builds its nest exclusively on the outside of buildings, and only when occasionally losing its way will it fly into the interior. Imported to America, the common sparrow acquired the habit of choosing for its winter quarters certain buildings (hothouses and pachyderm-houses in the Bronx Zoölogical Gardens, New York; in the hothouses of Lincoln Park, Chicago), where it even breeds, without ever venturing back into the open. Without the assistance of inheritance, such changes could hardly ever be explained.

Dogs and even cats of well-trained progenitors, although they were never taught tricks themselves, in-

herited this training—like "saying its prayer," "begging," "giving the paw." Hunting and police dogs are well known for their way of "pointing" and "retrieving." These phenomena belong within the scope of this classification much more logically than under the heading of selection or accidental change of the germ plasm.

4. *Anatomy:* The negative conception that selection is not creative brings us in close contact with the assumption that decidedly adapted forms were brought about "out of one's own power." In this category belongs all the protective similarities ("mimicry," in a wider sense) to the environment, be they color or form, motion or posture. Others belonging in this category are the practical adaptations of the organs of locomotion which, based on the same skeleton (for example, the extremities of the vertebrates), adapt themselves to specialized application, as for running, jumping, climbing, flying, and swimming.

Still other adaptations belonging in this class are made up of retrogressions of all such organs and tissues as are contracted by light and color in animals living in complete darkness, that is, living in the deep sea, in subterranean caves, in the trunks and stems of trees and plants, or in the interior of animal bodies (*Entoparasites*). If the latter as a rule are of bleached color and have atrophied eyes, others living in semidark locations (like the entrance of caves, in water which is still penetrated by light, or in deep sea locations which are slightly illuminated by phosphorescent organisms) or only emerging from their hiding places from dusk to dawn, are endowed with greatly

enlarged visual organs which are very sensitive to light, and are very often darkly colored.

Not only those eyes and pigments atrophied on account of darkness, but all those organs which retrogressed into a rudimentary state on account of non-usage, furnished indirect proof for the inheritance of acquired characteristics (Plate, 1913).—Spots on the skin which, in the course of life, are frequently exposed to pressure and friction, develop callosities and remain hairless. Such tendencies may be noticed in the embryo long before these spots are actually exposed to pressure and friction (For example: callosities on the human sole; Albinus, Semon, 1913.—Horny pads on the breast and knee of the camel and on the ankles of the giraffe.—The gluteal callosities in monkeys; Shattock, who, by the way, interprets his findings negatively.—The horny pads on the fore legs of the Wart-hog, *Phacochærus Africanus;* Leche).

Functional adaptations to be acquired in later life in the embryo are already prepared in the following two cases: The masticatory surface of the molars of the Dugong (Kükenthal); the remarkably inverted form of the stomach in bats, because the latter, when relaxing, hangs on with its feet, its head hanging downward. Brüel, in the course of his anatomical studies in reference to the nervous system of *Firoloida kowalevskyi,* came to the conclusion that the deplacement of the copulation organ to the right side of the intestinal sac and the accordingly changed nerve-supply (multiplication of the nerve fibers, especially on the right side) could only be brought about by a changed mode of use.

5. *Embryology:* Every embryo, from egg until the

attainment of full development, quickly passes through all those intermediate stages which in its forebears already amounted to final stages. Without an inheritance of acquired characteristics, these "biogenetic repetitions" would seem impossible (MacBride, 1917 b; Semon, 1916). For example: if every human embryo passes through a stage where it develops gills and fin-like extremities, such a reminder of the aquatic stage of its forebears is only possible because adaptations to an environment entirely different from the present cannot be lost so easily, but rather became fixed by heredity.

The hereditary monstrosities of Japanese goldfish breeds (Chapter XIV) are induced by "plasmic weakness" (Tornier, 1908; Milewski), as a result of inferior conditions of life. Similarly, all the other much-wondered-at products of Far Eastern animal breeding and plant raising are not the result of strict selection, but are effected by direct, individual variation.

6. *Palæontology:* Those examples of adaptation as mentioned under "Anatomy," and which could not have been effected by selection, frequently are rounded out by palæontological documents. Palæontologists (among them Cope, Osborn, Abel) never denied the doctrine of the inheritance of acquired characteristics as strongly as the biologists. Some of the palæontologists, in affirming this doctrine, go even much farther than the most positive biologists, among them O. Abel, who in his "Palæobiology" without reserve assumes that the inheritance of mutilations is indispensable for evolution.

Especially instructive are the adaptations of the

organs of locomotion in consecutive periods of geological evolution—the change from walking on the sole to walking on the toes (simultaneously with a progressing reduction of the number of fingers and toes) for the purpose of quicker movement on solid and level ground. This adaptation, analogous and independent, may be observed in as widely differentiated groups as in horses, ruminantia, jumping rodents, jumping marsupials, in dinosaurs and ostriches.

7. *Geographical distribution:* Regions of the earth's surface with certain geological or climatic characteristics, if inhabited by living beings at all, always manifest a correspondingly characterized fauna and flora, at the Polar regions, deserts, mountains and lowlands, rivers and lakes, shores, deep sea, etc. The mammals and birds of North America, from the north and east towards the south and west, gradually become of lighter color and grow smaller in size (G. M. Allen). The same holds true in reference to the Colorado Potato Beetle, *Leptinotarsa decemlineata* (Tower, 1906). The terrestrial forms of snails, inhabiting the island of Celebes (P. and F. Sarasin), and a form of *Cerion* snails to be found on the Bahamas (Plate, 1907) constitute "form-chains," and in the direction from west to east manifest regularly graded changes.

Wherever animals and plants from related forms are separated (by bodies of water, mountains, valleys, vegetation zones, which do not conform to their necessities of life) localized forms evolve. These are the more differentiated from the original forms the longer ago the separation was effected.

Localized forms of this kind are most strikingly il-

lustrated by the flora and fauna of small, distant islands. On the Galapagos (Charles Darwin, 1883), on many archipelagos in the Pacific Ocean, on the Seychelles in the Indian Ocean, and so on, the development of localized forms has progressed to the extent of recreating new species, genera and families. On the "Scoglias" of the Dalmatian archipelago, however, because of not having been separated from the European continent so very long ago, species are found identical with those which inhabit Dalmatia and Italy. It is remarkable, though, that on each one of these cliffs new varieties have developed which are different from the original form (Kammerer, 1918). This same process of hereditary adaptation to local conditions (which, on islands separated long ago from the continent, has progressed so far as the creation of local species) is only in a *statu nascendi,* in reference to newly separated islands.

CHAPTER XXXIX

SUMMARY: FACTS AGAINST THE INHERITANCE OF ACQUIRED CHARACTERISTICS

A

Direct or Experimental Counter-evidence

The following artificially induced changes were not transmitted to the progeny of directly changed progenitors:

1. *Locomotion:* Payne, for forty-nine generations, bred the fruit-fly, *Drosophila ampelophora* (Chapter XVIII), in complete darkness. There were no changes as to the body, but beginning with the tenth generation it was observed that while the normal specimens always made for those places which were bathed in light, specimens bred in darkness gradually became more and more shy of light. Their offspring, even when bred in daylight, seemed not so much inclined to prefer light spots as specimens of "normal" progenitors. Payne, in an additional paper, renounced this positive assertion, after having bred sixty-nine generations of the fruit-fly in the dark and after having carefully measured the frequency and strength of their tendency to seek the light (positive phototaxis).

The twirling of those rats which were taught by Griffith and Detlefsen to counteract a rotating move-

ment, was not genuinely hereditary, but due to bacterial infection in both parents and offspring (perhaps, the Japanese "dancing mice" are a second example of this kind).

2. *Periodicity:* In Germany the summer wheat *(Triticum vulgare aristatum)* needs about one hundred days for maturing. Schübeler imported it to Norway and noticed that in its third generation raised in the north this wheat needed only seventy-five days to mature, because in Norway the sun, from the middle of May until the end of August, shines for longer hours than in Germany. Now, a part of the crop was restored to its original habitat (Germany), while another part was planted in Norway. The result was the following: The wheat in Norway, just as in the years before, matured in seventy-five days, while the same wheat in Germany now needed only eighty days to mature.

This positive result, even though very cleverly defended by Semon, 1912, 1921, could not be upheld, especially after Wille had expressed his opinion on this case. The wheat which Schübeler used for his experiments was a mixture of varieties, some of which would naturally mature earlier and some later. Now, while there is more probability for the former to be harvested in Norway, the latter has a better chance to be harvested in Germany. The result, for this reason, had to be considered as effected by selection and not by adaptation.

3. *Size:* Differentiations as to size, climatically acquired by plants either raised in mountainous regions or in the lowlands, are not always and under all circumstances hereditary (see, also, facts in favor of the inheritance of acquired characteristics, Chapter

XXXVIII, A, 9). There are species where each specimen, according to its individual inclination, can adapt itself to mountainous or lowland sizes in accordance with the location allotted to it. Take, for example, a dandelion seedling (*Taraxacum officinalis*) and cut it in halves lengthwise. While one half, sowed in mountainous regions, develops into the mountainous form, the other half, planted in the lowlands, develops into the lowland form; each looks so decidedly unlike the other that they may be taken for entirely different plants (Bonnier).

Differences as to size in beans, effected by being differently situated within the shell and for this reason differently nourished, are not inherited. All consecutive generations again produce all the sizes, independent of whether the smallest or the largest beans were used for seed (Baur, 1913).

4. *Forms:* Mutilations, injuries, maimedness effected by mechanical means, are not inherited. The shortening of the ears and tails of certain races of dogs and horses, the puncturing of the lobe to wear earrings, circumcision as practiced by the Semites, defloration, deformation of the feet of Chinese women, etc., do not leave hereditary traces in the progeny. To be sure, once in a while dogs and cats with shortened tails are born, or humans with an atrophied prepuce, but this can also be observed in races and families where nature was never tampered with.

In this class belongs the celebrated experiments of Weismann of cutting off the tails of mice (1882; Weismann's collected essays, 1892), a case which up to today the opponents of the doctrine of an inheritance of acquired characteristics draw upon as the highest

court. Usually scientists refrain from quoting results of investigations of so long ago, for the purpose of substantiating the latest developments of science, especially when—as in the famous Weismann case—extended and varied controlling experiments were never performed (except Ritzema Bos). And not only mutilations as such, but also the special form of the regenerated part (Przibram, 1909) or additional parts (Tornier, 1896) do not manifest hereditary effects.

The same change from low helmets to high ones, from a short abdominal sting, adjusted at a sharp angle, to a long and straightened-out sting, observed by Woltereck on *Daphnia longispina* after keeping this water-flea in raised temperature for the duration of two years, Wolfgang Ostwald effected in a related genus, *Hyalodaphnia.* The result did not prove to be hereditary, most probably because Ostwald did not continue the experiment long enough.

5. *Color:* The same discolorations, which MacBride and Jackson, Przibram and Brecher (see facts in favor of the inheritance of acquired characteristics, Chapter XXXVIII, A, 8 b) had observed to become hereditary in the stick-insect, *Carausius* (*Dixippus*) *morosus,* Schleip did not succeed in proving as hereditary. In his estimation, only the ability of each generation (and of each individual) to acquire those changes, according to necessity, is hereditary.

There is a race of the Chinese primrose (*Primula sinensis*) with white blossoms. But as to its determinants, this white race is different from the variety raised in hothouses where the blossoms, ordinarily purple-colored, whiten. This change of color is not passed on; for, when restored to low temperature, the

new blossoms will be purple again, whereas the white variety, in all temperatures, remains white (Baur, 1913, 1920).

An identical discoloration which, in reference to the eye Kapterew observed becoming hereditary in the water-flea, *Daphnia pulex,* when kept in the dark, Papanicolau also observed in the light and, for this reason, assumes the discoloration to be nothing more than a general symptom of degeneration, not depending on exterior influences.

6. *Diseases:* Analogous is the criterion of Maciesza and Wrosek, who tested the experiments of Brown-Séquard, even though they admit that the inclination to epileptic attacks is intensified in the progeny of such guinea pigs as were made epileptics by operation. Entirely negative results, when testing Brown-Séquard's experiments, were reported only by Sommer, who availed himself only of very limited material.

7. *Immunity:* A certain resistance of the progeny of immunized mice (Ehrlich) against Ricin and Abrin; against tetanus and hydrophobia in mice and rabbits respectively, immunized against these diseases (Tizzoni and Cattaneo); against diphtheria in rabbits immunized accordingly (Behring), could be observed only in those cases where both of the parents had been immunized—or at least the mother, but not alone the father. Identical results yielded experiments with rabbits, which were immunized against an extract made from the liver of guinea pigs—a foreign albumin which, under normal conditions, should have proved to be poisonous to rabbits (Charrin and Delamare). That in all these cases, immunity is not transmitted from the father points to the fact that the transmission

is not effected by the germ cells, but rather by the placenta and by way of lactation. Birds, which neither have a placenta nor depend on lactation for nourishment, never manifest a transmission of acquired immunity, as shown in Lustig's experiment with poultry immunized against Abrin (compare facts in favor of the inheritance of acquired characteristics, Chapter XXXVIII, A, 11).

8. *Mendelism:* For reasons not to be repeated here (because being of strictly theoretical nature, they do not belong within the scope of a chapter dedicated as far as possible to nothing but a summary of *facts*. Compare, however, Chapter XVIII), there is a strong tendency toward drawing upon the Mendelian Rule, observed in race crossings, as evidence against the inheritance of acquired characteristics. When opponents of this doctrine speak about the "conclusive evidence" *against* the inheritance of acquired characteristics, the results of hybridization is generally in the back of their heads.

But this is to be considered: In the first place, Mendel's crossing experiments had nothing to do with acquired characteristics. They were merely experiments in reference to the inheritance of *inborn* characteristics and, therefore, cannot shed light on the problem of the inheritance of acquired characteristics. To prove a phenomenon to be impossible, it is indispensable to give this phenomenon *per se* direct attention.

In the second place, it is impossible to prove an impossibility and such a "proof," be it observed ever so frequently and even if it is one of those "old established experiences," is usually considered null and void

as soon as only one single affirmative evidence has been produced. The present case is not even one of those where a few positive experiences face any number of negative observations and where, in spite of the numerical strength on the negative side, the positive side would have to be preferred. The overwhelming majority of experiences regarding the inheritance of acquired characteristics is of the positive kind, at least as far as systematic, scientific experiments are the source of these observations. That this source is the most important has hardly ever been denied by any scientist!

Finally, the alleged counter-evidence based on the Mendelian Rule may be considered disposed of, because there are acquired characteristics which, after becoming hereditary in crossing experiments with unchanged specimens, are subject to the Mendelian Rule of Segregation (experimental forms of the Potato Beetle, *Leptinotarsa;* Tower.—Midwife Toad, *Alytes;* Kammerer, 1910 c; 1911 b.—Tendency of the fruit-fly, *Drosophila,* to grow additional extremities when kept in cold; Hoge).

9. *Xenias:* The double fecundation of seeds which imparts to the endosperm—for example, when crossing yellow (maternal) with blue (paternal) maize—the blue color of the paternal race is considered experimental counter-evidence against the inheritance of acquired characteristics. How this interpretation is reasoned out was shown in Chapter XXV, where it was also shown why these spurious "xenias" of the vegetable kingdom in reality do not prove anything against the inheritance of acquired characteristics. There are genuine "xenias" of the animal kingdom. They were

discovered by A. v. Tschermak (1910), while crossing different races of finches and poultry. Here, a product of strictly maternal origin, the shell of the egg proved to be influenced by paternal characteristics. This can only be explained by relayed induction of the characteristics from the paternal to the maternal tissue. With each one of such cases of somatic induction, the genuine inheritance of acquired characteristics becomes clearer to our insight.

10. *Grafting:* Seemingly, the assumption that "stock" and "scion" (Chapter XXIV) do not influence each other's characteristics does not manifest any such relayed somatic induction as would be a necessity if genuine inheritance of acquired characteristics is supposed to be possible.

Especially, wherever generative glands are ingrafted on foreign races and yield progeny, the independence between "scion" and "stock" offers a strong bulwark against the inheritance of acquired characteristics. Wherever generative glands of different races were successfully and thoroughly exchanged with each other, the progeny followed the origin of the "scion" and not the "stock" on which they were raised. Does such a specialized case of the independence of parts ingrafted on each other also prove the independence of body and germ? If so, it would be the strongest and most direct evidence against the inheritance of acquired characteristics.

However, the experiments of exchanging parts of bodies dealt only with old, fully established races for material, and not on those races which are not considered to be fully established, as newly acquired characteristics are still to be observed in them. Regarding

grafting, the same holds true as in reference to crossing: if something is to be ascertained about the inheritance of acquired characteristics—be it in the positive or in the negative sense—the experiments have to be conducted on acquired characteristics. This perfectly logical condition up to the present time has only been fulfilled in regard to the experimentally acquired striped design of the spotted salamander (Kammerer, 1911 a, 1913 c), when the ovaries were exchanged. In these experiments, the progeny raised from a transplanted ovary proved to be influenced by the body of the foster-mother.

But, aside from the transplantation of generative glands, the mutual independence of ingrafted parts is subject to exceptions, the number of which is continually growing (Chapter XXIV). For this reason, conclusions drawn from grafting experiments can no longer be cited to disprove the inheritance of acquired characteristics.

11. *Chimeræ*: The chimeræ, or spurious graft-hybrids, do not come within the scope of the aforementioned exceptions to the rule of mutual independence of ingrafted parts. Graft-hybrids are mixtures derived by a process of ingrafting parts of the body on each other and do not result from a blending of the germ plasm. Quite frequently they proved to be nothing but just "chimeræ." The original tissues, of which these chimeræ were made up, remained of pure strain and only because of growing together—either vertically or horizontally—the impression of complete blending was created. The revealing of what these assumed graft-hybrids really were amounted to a victory for the opponents of the inheritance of acquired

characteristics, a brilliant affirmation of the transplantation rule, according to which "scion" and "stock" remain mutually independent, even in those cases where they apparently blend completely.

However, with every case where "stock" and "scion" influence each other, the chimeræ lose in evidential value as proofs against the inheritance of acquired characteristics. So much more, as one of those exceptions deals with graft-hybridism proper: *Solanum Darwinianum* is a genuine graft-hybrid of tomato and Common Night-shade. Its cells are not of pure strain, but reveal a mixture, because their own chromosomes are a combination of those of the paternal plants.

B

Indirect, Non-experimental Counter-evidence

If it is already difficult to prove something negative, such a difficulty is even intensified if the counter-evidence is only circumstancial, *i.e.*, indirect. If it is not to be substantiated by experiments, but rather only to be based on simple observations and deductions drawn from them, their strength of persuasion is naturally limited.

1. *Mutations:* Once in a while, distinct changes are to be observed, apparently without the environmental conditions of life having undergone a corresponding change. At first, only comparatively few specimens may show the changes, but as these distinct changes (mutations) are immediately and completely inherited, there is sufficient occasion for the new characteristics to multiply and spread.

After the belief in the inheritance of acquired characteristics (on account of Weismann's almost universally accepted theory of continuity of the germ plasm) had been shaken to its very foundations, when the second dynamo of the change of species—selection—was proven to be impotent to create and intensify characteristics, the discovery of the mutations by De Vries was hailed jubilantly, these mutations (as elaborated upon in Chapter XLI) remaining the only props of the whole doctrine of evolution.

But what was it that interfered with simply identifying the *new hereditary* characteristics (since called "mutations" by De Vries) with the "acquired characteristics" of the older evolution theory?

What interfered was the fact that the mutations—as mentioned before—seemed to be able to appear without being induced by corresponding environmental changes. Merely internal changes of the germ plasm, rearrangements of its most intimate, molecular structure were held to be responsible for these mutations appearing so suddenly. And inasmuch as this mere assumption, this hypothesis of an internal influence, was generally and unconditionally accepted as a fact, the doctrine of the inheritance of acquired characteristics had been rendered unnecessary.

As will be explained later on (in Chapter XLI), it is altogether wrong to say that mutations are independent from environment and are induced by nothing but internal influence. As has been experimentally and strikingly proven by Tower (1906), in reference to the animal kingdom, by MacDougal regarding plants, and by Jennings (1908-1920), Jollos and M. Wolf regarding protista, mutations are also induced by exterior

influences. This is also substantiated by a long series of simple, non-experimentally ascertained facts (Chapter XLI). An equality of mutations and of acquired characteristics seems thoroughly justified.

Moreover, the first mutations discovered by De Vries (1901), on the Evening Primrose, *Œnothera lamarckiana,* were not genuine mutations at all! These plants, with which De Vries started his investigations, eventually proved to be hybrids; and those plants of pure strain, as in the course of time segregated in accordance with the Mendelian Rule, were wrongly considered mutations. Exactly the same objection raised against most of the breeding experiments, in connection with the inheritance of acquired characteristics, are practically applicable to the first mutations, namely, the objection that a mixture instead of a pure type was employed for the experiments, and that for this reason apparently new acquired characteristics were only old characteristics asserting themselves once more.

Later investigations, however, proved the existence of mutations beyond doubt. Time and again in the history of science, it has been observed how, by way of errancy, truth was finally discovered (see Vaihinger's "philosophy of the 'as if'"). One of the most remarkable proofs of this is the discovery of the mutations.

2. *Geographical distribution:* But, if mutations are not induced merely by internal rearrangements of the germ plasm, but because of environment, how is it that utterly different hereditary variations (*i.e.*, mutations) are to be found in one and the same region, mixed among each other? There, one would reason,

they are subject to the same environmental influences and by necessity independent of them; and, therefore, must have developed independent of environmental conditions.

Just one example: the Common European Garden-snail (*Helix hortensis*) has either a plain yellow shell or a shell with black stripes on a yellow background. These stripes may be wider or narrower, more or less striking, and sometimes crowd out the prevalent yellow color until the whole shell is black. Aside from yellow shells, there are also red and whitish ones. Most of these colors, according to experimental proof by A. Lang (1909), are absolutely hereditary. After a warm rain, it is sometimes possible to collect all the differently colored shells along the same hedge. And from this fact, that one is not dealing here with a case of "local races," it could be deduced that purely internal causes are to be held responsible for these phenomena.

But only a poor logician would draw such a conclusion. The races (or in another case, the species) do not necessarily need to have lived always in the same locality. That they are now living together may be just the result of immigration. They could also have evolved at different periods when the environment was different. Especially if there is such a thing as an *inheritance* of characteristics acquired at different times and different locations, a later equalization of the original environmental differences must leave the acquired characteristics untouched. Exactly as the progeny of the mice, in the experiment of Sumner, and of the rats with which Przibram (1910, a, b; 1923 b) experimented, retained approximately the longer tails of their

progenitors when kept at a high temperature, and the shorter ones when kept at a lower temperature, even though they (the progenitors as well as the progeny) were later on kept together at an intermediate temperature; so the "cold" rats and mice and the "warm" rats and mice were easily distinguished from each other by the length of their tails.

Therefore, the opponents of the inheritance of acquired characteristics, for counter-evidential purposes, draw upon a fact which can only be a fact if there really is an inheritance of acquired characteristics and which could hardly ever be observed if there were no inheritance of acquired characteristics.

3. *Theoretical objections:* When Semon, in 1907, was busy collecting proofs for the inheritance of acquired characteristics, he had to contend only with the following four main objections:

(1) The objection of a direct influence of the germ plasm.

(2) The objection of selection.

(3) The objection of atavism.

(4) The objection of logical counter-evidence.

The number of the purely theoretical objections meanwhile has been enlarged. Today a more finely differentiated classification of the objections has become necessary, as follows:

(1) Objection of *direct* (physical, not physiological) *influences on the germ plasm.* The result of such an influence would not constitute genuine heredity, but only "pseudo-inheritance" or merely the after-effect of a non-hereditary *"modification."* Even "accumulated after-effects" (Alverdes, 1921 a), *i.e.*, a multiplication of modi-

fications through many generations is sometimes observed, but does not refute the objection raised.

(2) Objection of a parallel (non-somatic) *induction of the germ cells.* The result may be genuine heredity, permanent change of the germ plasm and its products, and is no genuine inheritance of acquired characteristics, but rather the inheritance of *"mutations."*

(3) Objection of *non-acquired characteristics,* but rather of "pure strains" (*biotypes*) emerging from mixed strains *("phænotypes"),* as the result of environmental conditions or even independently from them.

(4) Objection that *selection,* naturally or artificially, intentionally or unintentionally, in present or past effective, produced the assumed "inherited characteristics," because they were practical and therefore of "selective value."

(5) Objection of *atavism.* The "acquired" characteristic is not acquired, but was only hidden ("latent," see objection No. 3) and for this reason remained unknown. Its original form and its momentary resurrection may be nothing but the result of a process of selection (see objection No. 4).

(6) Objection of *"transgressive variability"* (Lang, 1909) or *"oscillatory mutations"* (Cuénot). No characteristic as such is inherited, but only the ability, in accordance with environmental conditions, to react to them in one direction or another (Baur). Therefore, if a characteristic reacts to an environmental change, this phenomenon belongs within the scope of the "Reac-

tion norm" (Woltereck). Again the change does not need to be a new acquisition.

(7) Objection of *logical counter-evidence* (Weismann). The inheritance of acquired characteristics is inconceivable; therefore, it does not exist.

Just as the preceding chapters served to refute all these objections and classes of objections, the ensuing part of the book will also endeavor to weaken them. For this reason, it seems unnecessary to enlarge upon theoretical arguments within the scope of this chapter, especially as the present chapter, being a résumé, should serve, as far as this is possible, primarily to furnish a brief summary of facts. To a certain extent, some of the objections take care of their own refutation; for example, the objection of logical counter-evidence.

After being informed of all the facts and interpretations which speak against the inheritance of acquired characteristics, I hope that it is admitted that I seriously tried to present this material as completely as possible. But even an *advocatus diaboli* could never succeed in presenting this negative material just as strong of evidential value, and just as abundantly as he would be able to submit positive material.

The attention of an unprejudiced judge is especially directed to the following fact: *those investigations which expressly concentrated on investigating the inheritance of acquired characteristics hardly ever yield negative results. Of breeding experiments principally identical to the classical experiments of Standfuss and Fischer* (firstly, inducting a change intentionally and, secondly, proving its heredity after restoring normal

conditions) *only very few did not yield positive results.*

I still remember what Baur said to me, when, in The Botanical Institute of the University of Berlin, I found him busy with Mendelian crossing experiments on the Snap-dragon (*Antirrhinum majus*). I asked him whether he had ever taken the trouble to test an inheritance of *acquired* characteristics. Evasively he answered: "I never noticed anything like that!" But simply because a certain phenomenon has not been observed and without having tested this thing for its very own sake, but, on the contrary, to stubbornly deny it, seems quite insufficient, from a scientific point of view. It illustrates the same point of view still prevalent among the opponents of the inheritance of acquired characteristics which, only a short while ago, Redfield (1923 b, c), held up to them with caustic criticism.

Later generations of scientists, above the present fashion and above the prejudices of the day, will be surprised to learn on what poor evidence negative assertions of such far-reaching importance could be based.

For decades we have been fed with negative "proof" that acquired characteristics are not hereditary. How such negative proof is qualified is shown in this chapter, which therefore again concludes with the affirmative. But in no case is negation sufficiently conclusive to halt research work. It is exceedingly important to continue investigations and accumulate material that may help us to solve our problem.

On the solution of the problem, whether acquired characteristics are hereditary, depends the answer to another equally important question: Does true progress of humanity exist?

CHAPTER XL

THE ORIGIN OF SPECIES BY MEANS OF DIRECT ADAPTATION

If the previous chapters have not been written entirely in vain, they should convince the reader of at least one thing: If there is still doubt as to the inheritance of acquired characteristics, the whole issue can only be one about *words*.

The celebrated philosopher F. Mauthner very wittily proved how, time and again, words make fools of us. How delusive the term "*acquired* characteristics" is, became evident when the argument regarding the non-inheritance of mutilation (Chapter XXVI) was in progress. How confounding the mere conception of an organic *characteristic* may be, became evident when Baur tried to make things clearer: Not the characteristic is passed on, only the ability to answer to the demands of life with this or that characteristic (Chapter X).

Should not, then, such a condition of utter confusion also prevail regarding the term "*inheritance*"? Term and conception are derived from an analogy of material riches, which, within the realm of human society, are passed on from owner to heir. When speaking of the inheritance of acquired characteristics, the conception of external riches simply serves to explain the passing on of internal riches from progenitor

to progeny. But, as metaphors usually leave much to be desired regarding exactness, we may also conclude that the metaphor of the conception of inheritance is somewhat ambiguous.

Previous to the publication of Charles Darwin's book on "The Origin of Species by Means of Natural Selection" (1859) and especially before the publication of his book on "The Variation Under Domestication" (1875), natural history hardly knew anything of inheritance. The older biology got along without this conception and did not consider the phenomenon of passing on certain characteristics from progenitor to progeny as a problem principally different from propagation or growth even. Propagation as "procreation of one's own kind" and "growth beyond one's individual measure" embraced inheritance; if the riddle of procreation and of growth was solved, then heredity also was no longer a riddle.

Even Lamarck never mentions heredity, though the doctrine of the inheritance of acquired characteristics originated with his "Histoire Naturelle des Animaux sans Vertebrés" (1805) and his "Zoologie Philosophique" (1809). Lamarck does not state that "all that the organism of an individual acquires, implants, or changes in the course of its lifetime," becomes hereditary, but he says that it is "conserved by propagation" (*"se conserve par la génération," Histoire Naturelle,* etc., 2nd edition, 1835, p. 152).

It is open to doubt whether the creation of an independent problem of inheritance was real progress. To distinguish between the process of procreation and the process of passing on characteristics doubtless was of a certain value and made for the ascertaining of those

treasured findings which, without such an analysis, would perhaps never have been discovered. But here there happened once more what has happened so frequently before in the realm of science. The advantage of a keen analysis is partly lost again, because it is not immediately followed up by synthesis, while differentiating between conceptions very often is confounded with differentiating essentialities.

It is a different thing to study the reappearance of characteristics of the progenitors in the progeny aside from the study of the process of propagation, or to assume that the reappearance of characteristics of one's forebears is something principally different from propagation as such. By misinterpretation, the term "Inheritance" is accepted as a fitting metaphor for what, in reality, is nothing more than a far-fetched comparison drawn from an entirely different sphere of conception. And this misinterpretation is to be blamed for the misunderstanding of that endless chain which life forges out of innumerable links. It leads on to assume tangible limits where only abstract lines of demarcation between individual and germ plasm, individual and generation, could possibly be conceived—obstacles the harder to understand and to conquer the less they are real.

The disadvantages of severing the special problem of inheritance from the general problem of propagation has only been referred to here, in order to shed additional light on the problem of the inheritance of acquired characteristics.

Glancing once more over the facts which, in the course of the nearly concluded first part of this book, were submitted there can hardly be any difference of

opinion about one point: that the power of environment asserts itself in changing a living being. With what permanency and in what mode of appearance these environmentally induced changes manifest themselves in the progeny is another question. But the influence of environment, as such, nobody will try to deny any more, be he prepared to admit an inheritance of these changes or not.

Let us remember only one example of such environmentally induced changes: the "blind" eye of the newt *Proteus* which changes into a well developed visual organ if the animal is exposed to suitable illumination. Let us remember the arguments which this example started. Taken for granted that lack of light in subterranean caves effects an atrophy of the newt's visual organs, the experiment was hailed as proof against the inheritance of acquired characteristics. Because the atrophied condition of the eye was no definite condition but could be changed, even in one and the same specimen within the course of five years and without taking recourse to further generations, it was asserted that the atrophy of the eye is nothing hereditary, otherwise the atrophy could not have been overcome.

The influence of the environment (in the foregoing case, darkness) might have had the power to induce the atrophy of the newt's eye, but never the power to fix this state of affairs hereditarily. The ability to develop seeing eyes remained with the blind cave-dwelling newt through thousands of years, through uncounted generations, to an almost unencroached-upon extent: the acquired characteristic of atrophied visual organs, therefore, was not inherited.

Attention was directed to this illogical way of rea-

soning in Chapter XXXI, as far as the possible conception of inheritance is touched by it, namely: the conception that inheritance is something real and utterly different from mere propagation. But let us, for once, follow this train of thought and see where we will land.

What would have happened if Mother Nature herself—as she once in a while is apt to do—had conducted an experiment similar to the one conducted in the laboratory? Then in the habitat of the newt Proteus, somewhere in the ponds or rivers of Carnia or Istria, specimens would have been discovered with darkly colored skin and distinctly protruding eyes. Doubtless, one would have described these Proteus as a brand-new species and a new name would have been tagged on them. Frequently enough it has happened that, on the basis of still lesser differences, not only new species, but even new genera were "founded"—as for example in the analogous case of the so-called "sow bug" (*Gammarus*) and the cave-dwelling *Niphargus*.

Let us assume that things would have happened this way, which scientifically is absolutely possible. As a matter of fact, referring to the possibility that there might be Proteus living at large, endowed with large eyes, the evidential strength of my experiment was simply denied (by J. Loeb, 1915; see also 1916, p. 326). Let us further assume that, after locating seeing Proteus, I had succeeded in changing bleached and blind newts to dark-colored, seeing specimens. Then, of course, the differentiation of the two species—of the cave-dwelling and the non-cave-dwelling newt—most probably would have been retracted. It would have been decided that these two species of newts were no

genuine "species" at all, but only "local varieties"—that is, they are only differentiated by such characteristics as are impressed upon them by environmental conditions. And as such characteristics are induced, as well as reduced, by environmental conditions and opposing environmental conditions respectively, they are not to be considered hereditary. It is assumed, however, that a "genuine" species is always in possession of hereditary characteristics.

The seeing Proteus would not have been the first "species" to make the experience of being stricken out from the list of species, because one became aware that its distinguishing characteristics were only the result of environmental influences which, in accordance with the prevailing opinion, at least, are not hereditary. To give only one example: In the *Flagellata* group of the *Volvocales,* after breeding experiments by M. Hartmann, former "species" were demoted to "local varieties."

The question is now: What remains of genuine species and how many of them are left, if one bases the whole system of natural history on such a conception? In the light of such a conception, all species—especially if regarded from a more removed, historical point of view—may be nothing but just "local varieties." Or did not all of them, without exception, start out as "local varieties"?

Is there not a possibility that all the characteristics, which distinguish the different groups and species, earlier or later, were acquired by environmental influence, by modes of living? And because we are now engaged in dealing with this problem, let us clear away more of those antiquated conceptions and join the

chorus which now proclaims that such characteristics cannot be inherited. Very well, then; there is no inheritance of acquired characteristics! Are those phenomena of "inheritance," which we observe in the specific characteristics of races, species, and groups of higher order, in the end nothing but disguised after-effects, which may prevail for a time, for a number of generations, but finally must give way to the changing influence of deviating conditions?

If—as was asserted in connection with the Proteus experiment—one hundred per cent of variability amounts to zero per cent of inheritance, and *vice versa, then there is no inheritance; neither an inheritance of acquired characteristics nor any other.* Because *every* characteristic may be changed, every one of them is endowed at least with a certain amount of variability.

Here the objection will be raised: This is just plain hair-splitting! We know of old inherited, so-called "group-characteristics," which hardly, or only to a very small extent, can be changed by exterior influences. A characteristic (as, for example, the spine) has reached an entropic, definite condition which—analogous to a dead volcano—cannot be stimulated into new developments and cannot be deprived of its dynamic equilibrium.

One would simply point at the limitations of changeability. As a matter of fact, the knowledge we have of breeding and its application to effect artificial changes frequently teaches us that a change may progress for some time without interruption, then suddenly offer resistance—and now everything seems at an end. This experience gave rise to the opinion that every characteristic may be changed only within com-

paratively narrow limits: that these changes simply move around a fixed center from which they cannot depart as far as they please. Therefore, variability never reaches that hundred per cent which would be necessary to entirely eliminate the conception of heredity.

But there are two things which may be raised against the opinion that variability is limited and *always* less than one hundred per cent.

In the first place, *time* should not be overlooked. It is not possible to make a bird out of a reptile, or a feather out of a scale. If this could be possible, variability would be fully one hundred per cent and unlimited. Apparently that is right! But perhaps these things could be accomplished, if we only had the necessary time?

Of course, it must be admitted that a change of species needs less time than was generally supposed by the older theory of descent. Far-reaching changes, as the extreme differentiations of pigeons, poultry, and dogs, were effected within the course of a few years; while formerly, for such a process of change, centuries were considered necessary. As long as it was believed that evolution is nothing but the sum of all those little steps which sifting selection had accomplished, uncounted year-millions were necessary in order to prevent such a conception of evolution from crashing to pieces.

Breeding experiments and the theory of mutation have convinced us that such changes can be effected much quicker. The prejudice of Haeckel (in *Alte und Neue Naturgeschichte*), as well as of all the other theorists of the older school of descent—that

it is not possible, by systematic experiments, to study the mechanics of the change of species—has been disproved. But just the same, for the change of whole classes, geological periods are required. It is expecting too much to demand that such periods shall be covered within the short span of the life of a single scientist, in order to admit the boundlessness of variability.

In the second place, it should be taken into consideration that variability may be limited in regard to single characteristics which we try to change, but not as regards the complex of characteristics of which a species is made up. Let us remember the color changes of the spotted salamander. The animal normally is black with yellow spots. By exterior influences (Chapter XVI), I succeeded not only in eliminating the prevalent black color, but also in eliminating the yellow design and, in this way, succeeded in producing uniformly yellow or black salamanders. It is true, it was the limit to effect changes after the skin of the entire animal was colored by either the one or the other pigment: the most (as to color) changed salamanders still remained salamanders. But if not only the color and the skin, not only one single or a few characteristics are changed to such a "limited degree" (*i.e.*, until the changes embrace all possible parts of the body), if all characteristics are offered such a change to such a "limited" degree, the salamanders would develop into a *new* species, and into even more than a new *species*.

There is hardly any, be it ever so fixed, group characteristic which, to a certain extent, will not react to certain influences. A feather cannot be made into a

scale. For this, there is not enough time and not enough knowledge of the way in which nature effected the inverted process. But the feather itself can be changed in its form, size and color (experiments with humidity by Beebe on pigeons, and others).

Within the vegetable realm, Klebs (1907) proved that even the most unplastic organs will eventually give way to forced changes, provided that the changing agents are brought to bear upon the subject long and strong enough. In the Japanese Stone-crop, *Sedum elatior,* the petals and the stamens, regarding number and form, are easily influenced by changed nourishment and illumination, but the pistils remain unchanged. Now, if the stone-crop, with its thick and smooth leaves suited for dry and arid locations, is submerged in water (but not too long, as the plant could not stand it), the pistils are no longer able to withstand such a radical change in the mode of living, so they also vary.

The eye is surely a very old possession of the vertebrate. Of all the organs, the eye could be expected to resist exterior influences. But it is possible just the same, as has been shown before, to change the characteristics of this apparently unchangeable organ. And not only that, J. Loeb (1915) succeeded in raising embryos with no eyes at all, from a species having well developed eyes (*Fundulus heteroclitus*), by adding to the salt water potassium cyanid and keeping it at a temperature of 28° F., or by fertilizing this species with the spermatozoa of a different genus (*Menidia*). And even more than that, Stockard (1909), by adding magnesia to the salt water, succeeded in effecting a blending of the two eyes of the same species. The sym-

metry, one symptom of which is an equidistance of the eyes from a fixed point, could very well be accepted as an unchangeable characteristic of the vertebrates. But in spite of this, it was possible to develop a big, cyclopian central eye instead of the two, equidistant eyes. Here the fancy of Homer has become fact.

Only the *teratology*—that is, the doctrine of deformations—reveals the whole extent of what uncanny beings life can produce. Whoever, facing these misshapen forms, would insist that such monstrosities belong to the far removed field of pathological variations, are unable to live for this reason and do not play any part in evolution, should be reminded of certain races of our domesticated animals, for example: the bull terrier, the King Charles, tumbler pigeons, and the frequently mentioned Japanese goldfish monstrosities.

And does not the deep sea fauna put everything to shame that the most far-fetched imagination of a sensation-hunting artist could produce, or that the inexhaustible imagination of nature is able to produce in deformities of the human body alone? Speaking generally, where does this breath-taking strangeness of the deep sea fauna come from? Is not this fauna composed of the same groups of animals whose representatives in shallower strata are not so remarkable at all—fish, crustaceans, echinoderms, molluscs, medusas, and so on?

Let us once more appeal to the lay mind and to the creative brain of the poet: The farther a region is removed, the more strange we assume its inhabitants to be. What has not fancy made of the "man of Mars"? And why must the man of Mars be so very different

from man of our planet? Is it only on account of the great distance? Hardly; but rather because everything living on so distant a planet is supposed to be subject to conditions decidedly different from those of our planet.

Who, traveling in a submarine, waking up one night and staring at the inhabitants of the deep sea, could not very well imagine that he had been suddenly transported to Mars or some other far distant planet? Here, a creatively adopting environment has achieved its most fantastic masterpiece.

Returning to the realities of exact natural science, excessive formations and deformations, a greatly diversified fauna and flora as is found in such a grotesque environment as is the deep sea world, should teach us this fundamental truth: *Theoretically, as well as practically, the changeability of living beings is unlimited.* This limitless multifariousness is of exterior origin. The characteristics whereby races, species, genera, families and classes are distinguished, somewhere and somehow were created by exterior influences. They are adaptations to the exterior world.

Only in so far as they were detrimental, impractical, and therefore only temporary, they were again eliminated by selection. Environment creates without considering the purpose. The "purpose" is, so to speak, only a by-product which develops with a living being fitting itself into its environment. Whatever proves of no value is cast aside in the struggle of life. Races, species, genera, families, orders, etc., in the light of this, and in the last end, are nothing but "localized varieties." There are *only* acquired charac-

teristics, because all inborn and inherited characteristics had to be acquired once.

Whoever still wants to hold to the fashionable opinion that acquired characteristics are not hereditary may do so. By this he eliminates—and most probably not without justification—the conception of heredity from the realm of biology. Perhaps inheritance is really nothing but a spurious problem which forced science to lose decades in round-about-methods, a linguistic *fata morgana* which made it necessary to exhaust all the weak spots of a lame metaphor.

Nothing is absolutely inheritable (one hundred per cent); not one characteristic, not one change of a characteristic, because none of them has or ever had a zero per cent variability. In characteristics and their changes, the law of inertia manifests itself, and for this reason, characteristics and changes, for a number of generations, produce after-effects and remain recognizable even under conditions unfavorable to them. Even if the conditions which induce the characteristic, as observed last, no longer prevail, even if these same conditions change into contrasting ones, a certain inertia of organic characteristics still remains noticeable. In the latter case, of course, they are relatively shortest lived.

On the other hand, characteristics will remain unchanged for any length of time, provided that the environment is at least neutral. Just as a ball, once started on level ground, will keep on rolling as long as it does not encounter anything in its path to hinder it, or as long as no other impetus gives it a new direction and a different speed, even so the stream of life

languidly rolls forth in the same direction as long as no other exterior agents force it to deviate or to split up.

The gain which this way of looking at things presents to us, is a gain in more than one direction. This point of view furnishes us, in the first place, with a general explanation for organic variations. And as the variation of living forms is on the basis of their transformation, we also gain a general explanation for evolution as a whole.

The main idea of this is not new at all. Often enough it has been repeated (for example, by Ewald Hering, 1876, and Richard Semon, 1909, 1921), in the form of the "universal, plasmatic memory" (mneme). What memory, habit, practice, adaptation is in the life of the individual, is called inheritance in the life of the species. But it is always the same thing, always the same general ability of living matter to preserve impressions as long as an environment permits. New, as to the expression of my basical thought, may be the attempt to reveal inheritance as a pseudo-problem and to get along without this conception which, only through Charles Darwin, was introduced into the realm of biology.

Another new phase is the attempt to substitute for the conception of inheritance and the somewhat strange conception of "memory," the conception of organic inertia. With this, a second advantage may become manifest, in so far as our biological thinking may approach physical thinking.

The conception of the universal, plasmic memory lends itself to frequent misunderstandings. This conception somehow plays with the idea as if mental

memories were retained by the living substance, just as conscious memories are retained by the cerebral cortex. As if these memories actually, and not only as a disposition, were transmitted to the generation. In this way, the plasma of every cell is accorded some sort of an individual intelligence. Hering and Semon kept away from such an interpretation, but there is danger, and, sure enough, the school of the "Psycho-Lamarckists" (for example, A. Pauly) already fell victim to it.

Using the term "memory" brings us closer to a psychological conception and removes us, for this reason, from natural sciences, but the assumption of "inertia" brings us closer to a physical conception, thus tending to simplify and unify our conception of what nature is. I do not want to assert by this that, by now, the organical change and inheritance has already been explained physically, or has become physically explainable. We are still very far away from even a physiological explanation. But by introducing consistent conceptions into organic life, explanation has become somewhat more possible. It proves to be subject to the same laws as unorganic life. Here and there, the great questions are the same, still waiting to be solved.

And this not only holds good in reference to the questions of adaptation and inheritance. For me, organic inertia appears to be a bridge leading into another wide realm of natural, scientific research and nature-philosophical speculation; a new world of the wonders of periods and series of ubiquitous repetitions in *Bios* and *Cosmos*.

CHAPTER XLI

WHERE DOES EVOLUTION STAND TODAY?

THE theories of Evolution and the Descent of Man maintain that the species of plants and animals, and this includes the human species, are not unchangeable; that they were not created at the very beginning in their present state, but that the simpler forms developed into the more complex ones. Therefore, in the last analysis, all forms of life upon our planet are really blood relatives. Nevertheless, at the beginning of the nineteenth century the doctrine of evolution, advanced mainly by Lamarck and St. Hilaire, was suppressed not only by the church but also by science (Cuvier). It was only the appearance of Charles Darwin upon the scene, in the second half of the last century, that first decided the victory of the doctrine of evolution. The question now is: Was this victory decisive?

What is the key to the descent of man and the change of species? According to Darwin (1859) the answer is: Adaptation and Selection. Forms of life are the result of environment; as the environment changes, so do the characteristics of the species. New characteristics are required and, under certain circumstances, are transmitted to descendants, thus proving the theory of adaptability. The acquired and hereditary characteristics may be suitable or not; the owners

of suitable characteristics are preserved, while those with characteristics rendering them unfit for nature's purpose succumb in the struggle for existence and are stricken from the list of the living, or at first are eliminated from the ranks of their equals or of those possessed of greater endurance. Adaptation, therefore, is really a creative, progressive principle; selection is its negative helpmate, a sieve separating the fit from the unfit. A change of Darwin's conception regarding adaptability and the inheritance of acquired characteristics manifested itself in his different writings at different stages of his life, but Darwin never went so far as to deny this form of inheritance.

It is significant that a reactionary trend developed in some of Darwin's successors who, though forced to accept the theory of evolution as a whole, removed therefrom the progressive principle, holding that the change in species is caused only through natural selection, that newly acquired characteristics are not hereditary, that they die with the individual (Weismann); also that selection *per se* is creative, since it not only preserves the characteristics that have stood the test but also strengthens them. However, the Weismannities leave unexplained the true origin of these characteristics.

Feeble attempts to explain new forms through crossing of species—that is, through purely internal causes independent of external surroundings—must give way to the view that in the germ substance are embodied all characteristics and predispositions. The puerile theory of pre-formation (according to which all future generations lay ready in the ovary of Eve) wrapped in a modern scientific cloak, celebrates its resurrection.

Then came Mendel's discovery of the rules of heredity, followed by Johannsen's derivations of "pure strains," both demonstrating that selection is unproductive and that it does not strengthen existing characteristics. With this, the second pillar of the Darwinian theory of evolution appeared to tremble. Is there nothing, then, in adaptation? Is natural selection also ineffectual? What remains, then, of the entire Darwinian theory? The characteristics of all forms of life (to be more exact, their predisposition in the germ plasm) combine and separate again according to unalterable rules. Aside from very limited fluctuations around a fixed center, the predispositions of these characteristics cannot become greater or less and cannot be changed at all. Just as innumerable masterpieces of music are assembled from a few fundamental tones, just as a few fundamental tints magically reproduce multicolored reality, so is the ability of the living world to assume different forms derived from comparatively few fundamentals.

The belief of Linné (prevalent before the ascent of Darwin) in the fixity of the species has now been resurrected, and has only been converted along more scientific lines into the theory of the unchangeableness of organic tendencies ("determinants"). According to modern interpretations, the natural capacity for blending and separating of characteristics (which inherently are unchangeable) deludes us into the belief of a certain changeability of species; but this changeability would be much too limited to bring about a development of species and still more limited to create even larger groups and classes.

The theory of evolution at the present time is point-

ing in that direction; it is returning to the theory of non-evolution. Celebrated biologists, like Kurt Herbst of Heidelberg and W. Bateson (1913) of Cambridge, openly deride the concept of evolution in their lectures. Stagnation is trump once more!

Only such a state of affairs makes it possible that, even in a country as progressive as the United States, where there is a decided atmosphere of advancement, public debates are held between the "fundamentalists" and the "modernists," in the course of which scientific evolution is seriously attacked and questioned, by means of enumerating *pros* and *cons* which open up a staggering chasm of know-nothingism and are preposterous—to put it very mildly.

That such is the opinion about evolution in ecclesiastical circles is not to be wondered at. But it is painfully surprising if such a movement succeeds in influencing the lawmaking bodies to such an extent that, in some of the states of the Union, instruction in biology has been suppressed, simply because, in the course of this subject, evolution was taught.

In my native Austria, also thanks to the resistance of ecclesiastical together with reactionary cliques of scientists and officials, we have not as yet succeeded in establishing biology in the curriculum of our schools. What, in some Austrian schools, is taught under the name of "biology" is a virtual sneer at the science, and is rather a science of death than "the science of life." Of course, biological instruction in Austrian schools is not the least tainted by teaching evolution. Now, what is more dangerous: A state of development not far enough progressed, as yet, or a state of retrodevelopment which, on account of the wire-pulling influence of

reactionary elements, like Bryan, Straton, and others, is forced back into a stage long since passed?

And there is only one ray of hope which can save the doctrine of progress—the discovery of the *mutations* by De Vries (1901-1912).

Now and then sudden striking changes appear in plants or animals, transforming the character of species, in a seemingly unaccountable manner. Since the environment had apparently not undergone corresponding changes, it was taken for granted that the mutations in these forms of life were independent of the external world; and that the changes in infinitesimal parts of the germ plasm (which could hardly be seen or traced) were responsible for the mutations. Here also the last causes of these changes remain unexplained and inexplicable; they result from unknown, inner causes which baffle research; and so there always remains ground for assuming the workings of a higher power, or a metaphysical, supernatural principle of creation. This is the psychological explanation of the popularity of the mutation theory.

The most frequent mutations in the species belong in one of the following groups; they appear after years of unusual climatic conditions, or show in living beings which have become domesticated and thus brought under fundamentally changed surroundings; or they appear in living beings which, dragged from their natural habitat, become changed in a new clime; or they appear finally in such forms where the changed environment is purposely created for experimental reasons. One of the best known "mutations" is that of the maple (*Acer*). This mutation was later found to be originally brought about by bacteria (Van der

Wolk), and eventually became permanent. These observations bring me to the belief, in which I am not at all alone, that the mutations are in no way independent of environment and that they break through when the surroundings may have possibly become normal again and remain so for some time. My own experiments strengthen this theory, which at the present time has taken the following forms:

"Mutations" are only what was formerly referred to as "acquired characteristics," newly acquired from the surroundings, perhaps only specially strongly marked and therefore especially conspicuous, new characteristics.

Darwinism, therefore, stands justified. As I express it in my *Allgemeine Biologie* (1920 a): "Evolution is more than the fairest dream of the last century, the century of Lamarck, Goethe, and Darwin; evolution is truth—sober, delightful reality. It is not unmerciful selection that shapes and perfects the machinery of life; not disconsolate struggle for life alone governs the world, but rather out of its own strength every creature strives upward towards light and the joy in life, burying only what is useless in the graves of selection."

B. EUGENICAL PART

CHAPTER XLII

DARWINISM AND SOCIALISM

DARWINISM, in its original form, differentiates between two impetus for the change in species: adaptation and selection. Let the conditions of life change, and the forms of life change accordingly. The changes may be accomplished in an expedient manner or inexpediently; the possessors of suitable qualities are preserved and have thereby the opportunity of transmitting their expedient characteristics to their descendants. On the other hand, the unsuited forms of life succumb and are struck from the list of the living, or at least of the equals (Chapter XLI).

A reaction among Darwin's successors has striven arbitrarily to separate the last part of this simple and evident doctrine and to give it out as the whole of Darwinism. If the struggle for existence is praised as a progressive principle, the weeding-out by selection as a productive instrument, then Darwinism becomes anti-Darwinism and the theory of evolution a doctrine of retrogression: living nature would be void of morals and every act of baseness under an appeal of the natural struggle for existence would be not only permitted, but also bid for.

All measures for human welfare, such as medicine and hygiene, according to writers like Ammon and

Fritz Lenz, are to be considered only as obstacles to progress. Schallmayer, Pearl, Wiggam, Conklin and Martins deem it a naïve optimism to expect favorable effects, in future generations, of such measures as the protection of the laborer, the shortening of working hours, the betterment of the economic situation, and the bodily, mental, and cultural training of the young. According to Haykraft, tuberculosis, venereal diseases, alcohol, and war are even "friends of the race" because they make for a weeding-out process. Tille praises East London (the quarter of misery, where the stranded ones of the big city vegetate) as England's national hospital, because there those unfortunates fall victim to vice, plagues and famine. Ploetz sees a danger in the growing protection which is being afforded to the weak, a danger which menaces the general strengthening of a nation. He established a "Society for the Ennobling of the Race" to limit the interbreeding of the races, and to this end proposed to deprive the descendants of mixed marriages of the civic rights. Hansemann views the infant mortality as a foundation for racial strength, and any measure to diminish this mortality he regards as a national weakening. He would assume the rôle of a modern Lycurgus, who, as the first practical "Darwinist," ordered the sickly children of Sparta to be exposed to death. According to the decree of Lycurgus, Goethe, who was apparently still-born, would have been exterminated as a menace to the race.

Depending on whether one has in mind the genuine Darwinism (the theory of evolution) or its caricature —the modern and fashionable mania of selectionism —the relation of Darwinism to Socialism must be dif-

ferent. Only to this theory of Natural Selection could Haeckel's words be applied:

"Darwinism is anything but socialistic! If one wants to determine the exact tendency of this English theory, one must recognize that it is only an aristocratic, certainly not a democratic, or least of all a socialistic, tendency. The theory of Natural Selection teaches that, among beings and plants everywhere and at all times, only a small, favored minority can exist and bloom, while the superfluous majority suffers want and more or less prematurely degenerates miserably. . . ."

Entirely different from this is the real Darwinism. Like socialism, it is a doctrine of "upward development" and must concern itself with masses, and not only individuals, or it misses its aim. In the light of such an interpretation, the theory of Natural Selection is not unsocialistic, for its war-cry, "let the best man win," eliminates the prerogatives of birth and money, of internal and external inheritance. Class struggle is a veritable struggle for existence: a race with mental weapons, without violence, a bloodless and a positive selection—the survival of the fittest. War is synonymous with negative selection, with the left over of the weak and halt; and alcohol (another one of the celebrated—by Haykraft—"friends of the race") very often allows the drink-permeated and alcohol-poisoned ones to survive. But are these corpulent, fatty-hearted, shrunken-kidneyed, generically-rotted examples a suitable basis for the betterment of the race? Such a "selection" may be considered suitable for a punishment of the nation and for a breeding of a race

of lackeys that begets worthless gun-fodder, but hardly to build up a "chapter for race-refining."

The genuine theory of evolution would have characteristics which indicate their future worth, acquired through deliberate labor. Progress by evolution always goes along with a division of labor. Different occupations go together with different abilities, which are not so much the cause as the effect of training in said occupation. The resulting inequality is, however, not unsocialistic. As a matter of fact only by bourgeois misunderstanding is socialism generally criticized for "equalizing," for a leveling of social strata. But, into the general social fabric of a commonwealth, every member should fit himself in such a manner as to contribute to the general welfare according to his gifts and receive, in exchange, whatever other members can best afford to give. This is the ideal of true evolution; I hardly think that it contradicts the ideal of socialism.

And, finally, genuine Darwinism would have characteristics, acquired through labor and division of labor, passed on to posterity by way of inheritance.

A deed is indestructible (Karma; Carlyle), but only good action can bear good fruits. The individual neither lives fruitlessly nor must he always begin all over again from the very beginning. There is an organic ascent which is measured automatically according to our mode of life, according to our actions and sentiments. Without an ascent facilitated by the inheritance of acquired characteristics, the struggle of the disinherited and deprived ones would be without prospects. This theory of heredity is more moral than a theory of deliverance which threatens with temporal

and eternal punishment. This belief in evolution is indispensable for the workingman's movement, which has disposed with all other faiths, if his development is to grow out of his own strength. It must not be allowed to be shattered by a distorted, alleged Darwinism.

Among those characteristics that can be acquired and passed on, Darwin himself gave special consideration to the gregarious instinct. The urge for companionship is a lucky hit in the struggle for life (see Chapters XXIX and XLV). It is so advantageous for the preservation and progress of the species that its practice outweighs the disadvantages which its use inevitably brings with it. The ballast which a race drags along with it, in protecting incurables and cripples, is balanced (according to Bölsche) by the working out of the social instinct it affords to society. No victory in the struggle for existence exceeds in value of evolution the triumph of the accomplishment of mutual help.

The unadulterated Darwinism teaches of the transmutations of the species, the metamorphosis and interminglings of races and classes, the power of the struggle for existence, and the even higher power of help to existence. Far above its interpretation as a theory of higher evolution, Darwinism is a doctrine of natural, world-embracing humaneness.

CHAPTER XLIII

INHERITANCE AND RACE THEORY

> Strange is that our bloods,
> Of color, weight and heat pour'd all together
> Would quite confound distinction, yet stands off
> In differences so mighty. . . .
>
> —*Shakespeare.*

NEWBORN negro children have skins that are no darker than the adult Italians' or Greeks', but become dark in a few weeks. Egyptian babies require three years to develop the typical skin coloring. In the case of Southern European people, the color of whose skin does not have to change so radically, but one year is necessary. The soles of their feet and the palms of their hands being less exposed to light always remain paler than the skin surfaces, which are constantly exposed to the sun rays.

The foregoing observations (reported by Friedrich Hertz in his remarkable book, *Rasse und Kultur*) give us the key to that curious offspring of science, the race theory. This theory maintains that there are irreconcilable differences between races, nations, and classes which are an inheritance from the past. It seems to be the tool for creating national and class hatreds, but, at the same time, it is used sanctimoniously to rouse patriotism in the breasts of citizens of racially mixed states.

The race theory boasts of the rigid unchangeability of national character, but nevertheless professes to be convinced of the theory of evolution. Abusing the name of Darwin, the race theorists claim that the pitiless struggle for existence is their only guide, but at the same time they coquette with an international religion of love. The race theorists claim to shun the mixing of politics and science, but they creep around the throne and altar, and even sometimes "color" the truth for partisan advantage.

Pure races are extremely rare today, rarest in Europe's embattled soil where repeated intermixtures of the various peoples have made the distinguishing marks of race so confusing that they are almost illegible, particularly in the so-called cultured races.

Many Teutons, proud of their blond hair and blue eyes, may thank their Slavic or Finnish ancestors who once inhabited North Germany. Red hair was found as frequently among the ancient Teutons as it is found today among the Scandinavians and the Scots.

Purity of race must be preserved by inbreedings. It allows the long inherited, noble characteristics to be preserved, yet degeneration follows unless new blood mingles with the old, to preserve the merits of the ancient strains. The new blood also makes for the addition of new merits, offsetting the harm that inevitably comes to the old, undiluted strain by inbreeding.

Nature, by means of the migration of peoples, remains the breeder of races, despite the ideas of the race theorists. The northeastern part of France and half of Belgium are more German than South-Germany. German chauvinists frequently forget that

France was the land of the Franks, whose Teutonism nobody can deny. And French chauvinists overlook the Teutonic origin of many of their compatriots.

The distinguishing marks of former ancestry that may still survive the race-intermingling are effaced by environment. This is proved by the whites and blacks becoming Indianized in America (see Chapter XLIV), by Europeans in the Far East becoming Mongolized, by the lighter color of the descendants of Negroes who have lived in Europe for generations. Apparently, the same holds true for Negroes in the Northern United States. These facts have been aptly proven. The dark prairie Indians become lighter by settling in wooded areas. The light Indians of the woodlands become dark brown a few years after migrating to the prairies. The hair of the African colonists eventually becomes curly.

Since the race changes are more marked the longer the immigrant families remain in their new home (more marked in the children born there than in the first generation), it is evident that new racial characteristics are hereditary and that changes brought about by adaptability to climatic conditions are more easily preserved and passed on than lost. This brings me back to the beginning of this chapter where I said that newborn negro children have skins that are no darker than the skins of the adult Italians or Greeks, but that they become negroid in color within a few weeks. The explanation is simple. Evolution teaches us that the individual must go through all the stages of the development of the race (Chapters XXVI, XXX, XXXVIII, B, 5). That the negro race originally was not black is evident from the fact that the

newly born negro children are not as dark as the parents, indicating that in an earlier period of evolution negroes were not black, but migrated to a country where climatic conditions gradually caused a change in the skin's pigment.

Not only are racial marks that show on the surface of the body (such as the color of the skin, eyes, and hair) capable of adaptation, and of being transmitted under different climatic conditions, but even the skeleton may be influenced by changes in the environment.

There is a certain district in France where, according to whether they belong to the primary period of evolution or to the calcareous period, identical characteristics are to observed on the skeletons of human beings and domesticated animals. The race theorists, of course, from this would prove blood relationship. The miserable débâcle of craniology (*i.e.*, the science which investigates the dimensions of the skull), as long as the race theorists try to fix permanent racial characteristics by skull measurement, is largely explained by environment effects aside from crossing and selections.

For the longest time, craniology insisted on classifying long-skulled (dolichocephalic) races as superior races and round-headed (brachycephalic) races as lacking in moral and mental qualities. Despite this theory, negroes, the natives of Australia, and Eskimos have long skulls, while the old, cultured nations of Europe are brachycephalic. The round-skulls, by the way, always have a greater brain capacity, and school children in this group are frequently more intelligent than long-skulled children.

In the historical progress of civilization, the long-

skulled nations are almost always supplanted by the roundheads. It was found that when Egyptian mummies were unearthed, those with rounder skulls came from a more recent civilized period. Another frequent observation is that people living in mountainous regions are mostly round-headed, while members of city populations are prevalently long-headed, especially in the slums where the children of the poor very often fall victim to rickets.

The skulls of infants grow, as Walker reports, according to whether the young child rests with his head on soft pillows or on a hard support, becoming longer in proportion to the hardness of this support, and rounder, the softer the pillow is. The reason for this is that the infant naturally wants comfort and will lie on the side of its head in case there is no soft pillow. This eventually results in long-headedness, while a short skull will be the result of a soft support. These effects of the mere custom of caring for infants are often misinterpreted as racial characteristics.

How easily craniologists may be mistaken was proved some years ago by an incident concerning the finding of a group of skulls in the Potter's Field in Paris. The skulls differed greatly. A celebrated craniologist studied them and declared that they must be skulls of soldiers of the allied armies that conquered Paris in 1814. He claimed to have found skulls belonging to Finns, and Heaven knows what other "races," as the armies of the allies were made up of many nations. Shortly after he had pronounced his expert opinion, it became known that the skulls were those of the cholera victims of 1832. All of

them were skulls of French women, not of allied soldiers.

Because "superior" races could not be differentiated from "inferior" races on the basis of physical differences, the race theorists (for example, Chamberlain) started to harp on the mental and moral inequalities of all and any race they wished to antagonize. But race differentiations, based on mental and moral differences, stand the acid test of science even less, on account of being still more vacillatory than purely physical differentiations. For example: the rudiments of religion and law are similar in all races and only later on, even among closely related groups, develop along entirely divergent lines.

The greatest imaginable race-mixer is War and it is significant enough that Roman mythology begins from the rape of the Sabine woman. But otherwise, war does not produce the effect that the race theorists expect. Not the "superior race," not the "flower of the nation" is saved, but the weaklings, the old, and "the lame and halt and blind" (Kammerer, 1918 a). Statistics, referring to the European wars in the nineteenth century, prove that male children born in the years 1812 to 1814, 1871 to 1875 were least fitted for military service, whereas these same years yielded the greatest contingency of criminals. And, of course, there is no stronger condemnation of the race theory than the late Great War.

CHAPTER XLIV

INHERITANCE AND "MELTING POT" AMERICA

WHAT would a confirmed European race theorist have to say if he were given a chance to make observations in the United States? He would surely insist that here also certain race distinctions survived. The Negro, the Chinese, the American Indian (not so much the Jew) is still easily recognized. Their racial characteristics have defied a blending with the so-called Yankee type.

On the other hand, such a race theorist must admit that there is a distinct American nation—a young but already clearly defined type (at least as clearly defined as other nations, for instance Anglo-Saxons, French and Germans)—which, only in historical times, developed on the soil of the "New World."

The race theorist would also have to admit that the origin of this new, clearly defined nation is not at all a uniform one. Anglo-Saxons, Teutons, Slavs, the Latin nations, Greeks, and others, participated in this blending and eventually became—if they dwelled long enough in "the land of the free and the home of the brave"—"color-fast Americans."

Asked how he could account for this proven fact of blending, our race theorist would most probably explain it by resorting to the *crossing* of races: mixed marriages resulted in a mixture which only deceives

us into believing in an apparent homogeneousness, while the fundamental racial dispositions remain unblended and, at any time, are able to separate again.

The race theorist would deign to make such an admission because in doing so he may still resort to the interior agent of development, that is exclusively the hereditary qualities inherent to every race. *Exterior* agents of the new environment are not supposed to play any part at all. This still allows the provision that no real balancing was effected, but only mixture to which each race imparted its own characteristics, and from which each race may again withdraw these characteristics at any time.

There is no doubt that mixed marriages supplement the process of amalgamation. We need only look at the mulattoes and mestinoes to discern in them intermediate stages at which it is already so much easier to amalgamate and dilute the original races. But let us stop here for a moment:

Is the duration of such a process, stretching only over a few centuries, long enough to effect, by mixed marriages alone, a recasting of racial differences to a point where original differences are completely blended and where original multifariousness blends into homogeneousness? Stop and think: Were, in the course of four centuries, *only* mixed marriages contracted? Did not a natural gravitation of similar strata of society account for at least an equal number of race-conserving marriages? Let us admit that the melting process is, as yet, incomplete and that the "American race" is still in the making; then, whatever remains "physically un-Americanized" consists of the new influx which makes the melting process more difficult, detaining it, and

assigning to it ever new, non-converted material. But in spite of this, the result achieved so far permits us to deduct that the different origin of the parents cannot be held exclusively responsible for the result. We would also come to a negative conclusion if somebody would insist that an American race is the result of selection; essentially, only a type suitable for the New World which survived while non-suitable types went down in the struggle.

Every attempt to try to get along with crossing and selection alone would fall short on account of the following facts: The American is not simply a mixed type, combining the characteristics of original races or displaying them in a mosaic-like fashion; and Americans are just as little a "chosen people," *i.e.*, a predestined type of most suitable individuals. Doubtlessly, Anglo-Saxon characteristics prevail, but they do not prevail to such an extent that we could conclude that selection of the Anglo-Saxon type, at the expense of other immigrants, could be held responsible. The American type is rather a new creation, and the most remarkable thing about it is, perhaps, that certain physical and psychical traits of the American aborigines recur, or rather continue to develop.

There is no other way out than to assume that common environment is also to be held responsible for a leveling of racial characteristics. The *genius loci* (the spirit hovering over the locality) or, to express it less mystically, the total of all the energies of nature prevailing in atmosphere and food, in clime and soil, probably help to mold the humans that dwell there. Subjected to identical conditions, they gradually become

more identical to each other, as was to be expected on the basis of the original differentiations.

What the individual energies of nature are, which eventually amount to a process of leveling, is hard to discern. Such an analysis would be possible only by experiment, but as explained in Chapter III, humans cannot be subjected to such experiments. If animals and plants alternatingly are exposed to carefully graded conditions of temperature, light, humidity, and food, it is possible to discover which of the various influences are of a deciding nature.

For this reason, a manifestation such as we are dealing with in this chapter gains in lucidity when looking around for an analogy in realms of life below the species *homo sapiens*. Æsop and other poets knew this very well when they mirrored human life in animal fables.

The whale, a mammal as is very well known, assumed the appearance of a fish. Perhaps, because whale and some fish were crossed? We could just as well insist that the fish-shaped body of submarines is a result of a crossing! What is really at the bottom of these phenomena is that common locomotion in the water resulted in common peculiarities of form. The bat, flying saurier (*Pterodactylus*), and birds could not beget hybrids. The transformation of their anterior extremities into wings resulted independently and was induced by the intention to use the air for locomotion. Snakes, snake-like feetless lizards, and worms; ducks and the duck-bill; beaver, otter, and seal; kangaroo and jumping mice; humming birds and honey-eater, are a few other examples of *convergent*

adaptation. Here different lines of development all converge at a common terminus.

What we have already called the *genius loci* manifests itself especially strongly in parts of the world where many, even non-related, living beings are endowed with certain common attributes, although the latter may seem ever so incongruous to the individual species. On the Malayan Archipelago, animals, which nowhere else use the air for locomotion, acquired parachute-like organs. There squirrels, lizards, even frogs, and a certain species of insects, known as the "wandering leaf" (*Phyllium siccifolium*), are endowed with expandable "sliding planes" or have a flat body which enables them to slide down from the top of the trees. Among the mammals, birds, reptiles, and insects of Northeast Africa, the prevalent colors are a distinct brown, right next to a distinct white or a distinct black. In Surinam, the toads, snakes, and insects of many species are decked out with spots and stripes of flaming red.

All these phenomena are only explicable on the basis that there are certain physical peculiarities inherent to certain districts which influence equally all the living beings of this district.

There is another thing to be learned from "convergencies." Even though the flying squirrel, the flying dragon, and the flying frog of Java developed parachute-like "sliding planes"; even though the whale, the walrus, the seal, the manatee, the penguin, the sea-turtle, fish and fish sauriers (*Ichthyosaurus*) develop fins; even though the howling and spider monkeys, the kinkajou (*Cercoleptes caudivolvulus*), the tree-porcupine, the opossum, the chameleon, developed prehen-

sile tails; each of those different groups of animals did not become identical in all respects. They have a number of common attributes, but otherwise they retained the differentiations inherent to their origin. Only such differentiations become identical as an identical environment demanded. Of course, this process of leveling sometimes can reach so far that only dissection and embryological investigations succeeded in producing traces of different origin as in whales and seacows (sirenians).

As a rule, living beings subjected to identical conditions in identical environments only become similar but not identical. Analogous to this, an amalgamation of human beings, living in the same country, in the course of time, do not become one homogeneous race, but rather develop certain, common attributes. As an example, I mention the Jew who, in Western Europe, has a different appearance from the Jew of Russia and Poland. In England and America, the Jew also looks decidedly different from the Jew in East or Central European countries. Anybody who hails from Eastern Europe, where he is used to the type of Eastern European Jews, finds it difficult to distinguish between the Jew and Gentile in Western Europe. Later on, this distinction becomes easier, despite the fact that the Jew, in England as well as in the United States, assumed certain physical attributes of the Anglo-Saxons and the Yankee type, respectively.

That the unification of races, as a rule, does not mature into complete similarity should not be exploited in order to harp upon racial contrasts becoming indistinctable because we are able to view only a very limited span of time. Partial changes of racial

characteristics clearly prove that the respective races are not unchangeable. But even if only a part of these characteristics are exchanged for a part of the characteristics of some other race, this interchange finally amounts to a bridging over of contrasting attributes, eventually demanding additional exchanges, and finally making it plain that mankind are "all brethren."

Certainly, this amalgamation would progress even further, and finally reach an equilibrium where, even with the most painstaking methods, the origin of the different components could no longer be ascertained. However, up to now, we disregarded the fact that environmental conditions within a country are identical only as a whole, and not in their different manifestations. In the lowlands, other conditions prevail than in the mountainous regions, just as the conditions along the coast are different from the conditions prevailing in the *hinterland*. And all these conditions are again decidedly different from those brought about by the great cities.

In a certain district (as in the Ghetto or in Chinatown where individuals of certain races live together, or where are certain classes such as are found in New York's East Side, East London, Paris' Quartier Latin, living together voluntarily or under stress of economic or other conditions) certain peculiar types are not only newly created, but, on account of a certain topographical segregation, they remain decidedly more distinct.

The more differentiated the races are, the more difficult—and this seems only logical—their amalgamation, analogous to the flying dragon of Java which, in spite of the parachute-like connection between his extended ribs, cannot become as similar to the flying

squirrel as the flying dog, or the flying fox, or the flying maki, *Galeopithecus,* which (as mammals) are more closely related to the flying squirrel. Certain symptoms of approaching similarity are always discernible, however. There are (as mentioned in Chapter XLIII) any number of proofs of the bleaching of colored races (Mongols, Indians, Negroes) in parts of the world inhabited by Caucasians and *vice versa*—the white colonist of the Far East becomes "mongolized"; in Africa, negroid. In the latter case, the white colonist even grows kinky hair, while in the former case he develops high cheekbones and slanting eyes, depending on how long he lives in the country and whether he was born there.

Similarizations acquired in a common environment are hereditary. Otherwise, they would have remained "in the bud" with every new generation forced to acquire anew these similarizations, which most obviously is not the case.

I am concluding these observations with a question which to me, as an Austrian, is probably of greater interest than to the American, even though it teaches a very valuable lesson. The *antebellum* Austria-Hungary consisted of a conglomeration of races, more multifarious even than the United States at the present time. Germans, Italians, Roumanians, Jews, Czechs, Slovaks, Slovenes, Serbians, Ruthenians, Poles, Hungarians, and Cziganoes made up the old "dual monarchy." In spite of the fact that all these nations, with the exception of the Hungarians, belong to the Caucasian race, they showed only a very limited tendency to amalgamate into a pan-Austrian type. The German-Austrian made up one type and there was also

the German-Slav type, and a German-Hungarian type, mostly effected by mixed marriages. And all these types, to a tolerable degree, amalgamated with the Semitic type. There was no genuine unification.

If I am right in putting so much stress on environment, if the theory of convergent adaptability is correct, then how is such a contradiction to be explained?

It is to be taken into consideration that, not only in reference to component nations, but also in reference to component geographical conditions, the pre-war dual monarchy displayed great differences. The mountainous regions of the Alps, the lowlands of Galicia and Hungaria, the subtropical coast of the Adria did not furnish a well-balanced atmosphere which would have tended to level racial contrasts. Although within the boundaries of the United States differences are even more strongly marked, the area of this country is so extended that, in general, uniformity is prevailing to a much higher degree.

But as the different nations that made up the old Austria-Hungary mingled and generally gravitated towards Vienna, to permanently settle down there, why was not a uniform type evolved? The negative result most probably was brought about by the extraordinary nationalistic and racial hatred which, in the egoistic interest of individuals and for the benefit of party-political aims, was intentionally fanned. In this way, the nations separated more and more, becoming alienated in the same measure. The case of the dual monarchy was frequently cited as an example of how decidedly different, even antagonistic, nations could live together within a comparatively limited space without being permanently thrust into the throes of

war. But that the nations that made up the dual monarchy did not actually war on each other was the only contribution they made to the "peace of the empire."[1]

Anti-semitism, panslavism, jugoslavism, pangermanism, and other "isms," as a platform of independent and influential political parties, were happily nowhere known outside of the boundaries of the Old Austria-Hungary. Surely enough there is some antagonism in the United States based on racial prejudices; Chinese and Negroes are not liked all over the forty-eight states. But such prejudices exist only against the most alien races, and only the World War achieved the sad distinction of creating anti-semitism. But considered all in all, these prejudices seem on the down grade.

Identical environment is not enough to level racial distinctions. The good will to replace the struggle of life by mutual help is necessary if a durable and well-blended compound is aimed at. Such an achievement can be accomplished only when, aside from exterior influences of the environment, those influences make themselves felt of which Jack London, in his book, "Before Adam" (p. 85), so fittingly says: "Altruism and comradeship that have helped make men the mightiest of the animals."

CHAPTER XLV

INHERITANCE AND MUTUAL AID

In his utopian novel, *The Terrestrial Paradise,* the Russian author, Mereschkowsky, visions a generation living in consummate happiness. Surrounded by the inexhaustible, natural riches of the tropics, this generation leads a childlike life of innocence—carefree and self-satisfied. This new humanity, the product of artificial selection carried on by secret eugenic societies, arose from the old which had been weighed down by worries and whose decline is described along lines which present observations verify. In accordance with the endeavor of the secret eugenic societies, all those who seem unsuited to dwell in a terrestrial paradise are eliminated without the shedding of a drop of blood. A draught is given them which makes them sterile without endangering the life of the individual. Only beautiful humans, gifted with childlike joyousness, are selected for the Promised Island. They alone merit the never-changing joys of paradise—harmless, void of ambition, and ignorant to progress.

This terrestrial paradise, the way Russian authors and visionaries see it, is actually utopian; there is no place nor time for such a one. The choosing of the very best and most beautiful—even if it were possible to effect such a choice—would be just as futile as every other selection, as futile as any other driving power

which is exclusively rooted in nothing else but the struggle of life. There are other means of effecting the perfection of mankind (if the ignorance of the so-called "learned" and the spitefulness and egotism of politicians would not block the way): the inheritance of acquired characteristics as well as the give and take of the inevitable struggle for life (Charles Darwin), that is, the just as inevitable omnipresent mutual aid (Kropotkin).

The actuality of these two powers my readers will have to take for granted; for within the limited space of one chapter of a book there is no room for proofs. Those demanding proofs will have found them in the preceding chapters of this book (Chapters IV to XXXIX), as far as the inheritance of acquired characteristics is concerned. With reference to mutual aid, proofs will be found in my books (Kammerer, 1918 a; 1920 a). Those inclined to doubt may accept everything I am going to say now with the reservation, "provided this hypothesis is correct. . . ." But those denying everything had better stop reading right here. We are going to avail ourselves of a power (the inheritance of acquired characteristics) for the purpose of regeneration and the acquisition of some other power (the urge for mutual aid lying dormant) that, in the course of generations, they may bring about regenerative results.

Man, in contrast to other living beings which acquire and pass on characteristics, is endowed with an alert consciousness of his situation and his deeds. This alert consciousness endows him, within certain limits, with a "free" will; and only from this source there results the feeling of responsibility regarding what is, and

what is not, accomplished by man. For this reason, man is the only living being who, conscious of his goal, can aim at a higher development. And for the very same reason, we may rightfully expect that man will take care that those evolutions which humanity still has to go through shall consist in progressive adaptations; that is, in a diminishing of the burden which was passed on to us by our progenitors. We find this burden most difficult to carry, especially as regards the degenerative characteristics, by no means small, that were loaded on our shoulders by our forebears.

Whoever thinks that it is too much to expect that man in the future should not encumber himself with heavier burdens, but rather rid himself of this load—to whom such an alleviation and regeneration seems quite utopian—let him glance at the technical achievements of today. To the accomplishment of mechanistic technique, we will have to add the technique of life (Kammerer, 1920 b, c). Man has still to acquire sovereignty over the living matter of his own self, aside from his rule over the dead matter which surrounds him. Man must acquire the faculty to mold his pliable body and brains according to his constructive urges. And as it is an old and sad experience that always the most miraculous achievements of man's inventive genius are immediately placed in the service of the god Mars (submarine, aeroplane!), man has furthermore to learn to change from the destructive bent (mostly attached to the achievements of invention) to a constructive bent.

There is an inheritable regeneration: we may, we can, we must apply it! But there is another characteristic manifestation whose acquisition and passing

on by way of inheritance (or its preliminary condition) we frequently observe in the different kingdoms of creation (protista, plants, and animals; in higher and lower living beings) and that is the urge for mutual aid. This social stimulus—or to express it practically the same—this ethical will adds to individual regeneration, regeneration in general. The individual will to help gradually develops into the general will to help. *Symbiosis*, or the organization for mutual aid between individuals, develops into *pansymbiosis; i.e.*, an organization for mutual aid *en masse*.

As far as man is concerned, this ethical will *en masse* has still to be developed—this social consciousness of aim which is already dawning in much inferior strata of life where cell joins cell instead of following the individual courses of life singly (Chapter XXIX). We again find this same manifestation in higher developed strata of life where commonwealths of animal cells combine with those of vegetable cells to mutually improve conditions of life. But how shall we awaken this accumulative social will and thereby possess ourselves of the organic *media* of hereditary regeneration?

One of the most outstanding agents to achieve this aim is education. Education of oneself, setting good examples, enlightenment—all this together amounts to one of the most striking organo-technical means to achieve valuable regenerative characteristics, among them the instinct for mutual help and the instinctive yearning for peace.

With this, we have finally and completely collected the material out of which we may build our biological technic and ethics, the tools with which to erect our

belief in the future and humanity, when technic and ethics will no longer be unbridgable contrasts.

The education towards mutual help, the education towards peaceful endeavor, is the most essential material. The solidly founded assumption and expectation that the developed and individually accepted urge for peaceful help will become an organic heritage and so eventually amount to the possession of nations, races, and with that, humanity, is next to be considered. The third and last is the acquired instinct towards peaceful help as a way to hereditary regeneration, which will deliver us from the barbarian curse of militaristic accomplishments.

The password, so pregnant in meaning for the coming biological age—the age of upward development, if it is still in store for us—will be: Hereditary regeneration through mutual help in evolution!

CHAPTER XLVI

INHERITANCE AND AGRICULTURE

THE relation between man and his domesticated animals and plants is one of those symbiotic conditions which, in the course of this book (Chapters XXIX, XLII, XLV, and touched upon at the end of Chapters XLIV and XLVII), have been mentioned. In his capacity as a part of this symbiotic condition, man's welfare is closely connected to that of his symbiotic partners. Therefore, it does not seem out of place at all to dwell on the relationship between practical breeding of domesticated animals and practical raising of domesticated plants to the inheritance of acquired characteristics, within the scope of the eugenical part of this book.

I am neither a practical breeder nor an expert farmer; about the raising of domesticated animals I know just as little as about the raising of crops. I deem it wise to make this admission right in the beginning in order that my readers may know exactly how they stand with me. I have, as previously related in this book, bred entirely different animals—frogs (Chapter IX), salamanders (Chapters XV and XVI), and lower animals living in the sea (Chapter XXII). But what I have observed concerning these lower animals also applies principally to the breeding of higher

domesticated animals, and would even apply to the breeding of human beings.

As far as I am informed, the prevalent method of breeding still consists in the application of *selection* (Wilkens, Kronacher). If, for instance, the intention is to produce a cattle breed which yields great quantities of milk, those milch cows are selected for breeding purposes which either yield large quantities of milk themselves, or come from a stock known for its milk production. A bull is chosen from the same stock. If both bull and cow be chosen from a stock yielding great quantities of milk, it almost seems reasonable to conclude that they may be depended upon to intensify milk production.

But how does one know that the animals selected will surely produce female offspring of great milk production? This can be proven only by *one* test—the milking. As far as such selected cows are concerned, one may be *absolutely* certain that they themselves are the best milk producers, because the proof of the milking—is in the pail! For this reason most probably they may pass on this capacity to their female offspring, provided they are mated with a suitable bull.

Now let us see what happens if this method is followed up for a number of generations. A milch cow of relatively great production is picked as a specimen suitable for breeding purposes, to produce cow-calves of similar extensive milk production. Of these offspring, again the best milch cows are picked for breeding purposes. Are we really dealing here with selection as the only effective agent? Does it not seem almost unavoidable that application, *i.e.*, the contin-

uous "training" of the desired capacity (for great milk production) plays a prominent part here?

To be sure, this is exactly what I think, and my opinion is substantiated by my breeding experiments. Training and application, in regard to breeding, is the main factor; selection is merely incidental. The results of application are passed on, even if reduced, to the next generation. But this generation, well prepared for the task, gains additional training so much easier, and this training again becomes hereditary. In the course of a few generations, a quite noticeable "summing up" of the desired results is achieved.

By selection, this "summing up" of a desired characteristic—in this case quantity production of milk—is only supported and accelerated. However, even without selection, this process would come about only more slowly. There is another thing to be observed closely; and that is: selection neither effects nor intensifies the characteristic desired by the breeder. Appropriate exploitation, application, feeding, etc., always effect creative and intensifying results, and selection plays only the part of an auxiliary force more and more to bring out the result achieved so far, and more and more to refine its inadequacies.

If selection were the deciding factor, it would not be proven time and again that a certain highly developed breed will thrive only within a certain territory and nowhere else. Of course, it is possible to stock dairy farms with a superior breed in a different section of the country, and this expatriated stock may even develop higher. But under all circumstances, certain characteristics of the stock will change in spite of the breeder being ever so careful and ever so much

interested in wholly preserving the old characteristics; these characteristics change under his very hands. Even stricter selection will not serve to check this change, and the only way to preserve the original characteristics is to continually import new breeding material from the original habitat. The characteristics of the stock cannot be preserved by resorting to offspring born in the new territory. The stock gradually loses those characteristics for which it was renowned in the beginning and, under favorable circumstances, acquires new, valuable characteristics. In a comparatively short time, with or without selection and sometimes even directly in spite of it, the formation of a new breed within a new territory is accomplished. No doubt every breeder is in a position to furnish specific examples of what I have broadly stated here. And not only will a cattle breeder be able to do so, but also the breeders of horses, dogs, and poultry, as well as experts in raising farm products of all kinds, including fruits and flowers.

Unfortunately, the real reason for this generally known phenomena is clear to too few people. The driving power of race development does not rest with the breeder, in whose hands it apparently seems to be, but within the animal or plant, respectively. *The animal again receives the race-developing power out of the environment;* partly quite passively because clime, condition of soil, and fodder play unnoticeably a strongly influencing part; partly because the animal itself plays an active part in the change of the characteristics of its own race, because a changed environment necessitates a changed use of its organs.

The latter development may be most clearly ob-

served in race horses. The demands made on a race horse, for the purpose of racing, make it necessary for it to use its muscles in an entirely different way than a dray horse will use its muscles; and all the other non-muscular organs have to adapt themselves.

Is it possible to tell us the fairy-tale that all the differences between the race horse and the dray horse are brought about only by selection and without any hereditary influence of application? Could it be possible that the lifelong, incessant, hard training to which a race horse is subjected, which changes its whole make-up in the direction of a desired aim, and to which it finally adapts itself admirably—could it be possible that these changes, brought about by application, were without the slightest influence in the propagation of the stock? Or are these changes of such limited influence that they may be brushed aside and supplanted by a factor so deciding as selection is usually held to be? An old superstition is working havoc here which, in the interest of efficient breeding, ought to be done away with.

To be sure, as a rule, no such horses as run races are used for breeding purposes but only close relatives of them which are carefully chosen for the purpose of breeding. In the same way not those cows may be chosen for breeding purposes which, on account of quantity production of milk, are too valuable to be spared, but specimens of the same stock which are intentionally set aside to propagate a superior breed. But just as one can test this breed only by its milking results, to ascertain whether the whole breed is efficient in this direction; so, as far as race horses are concerned, racing itself yields only the necessary mate-

rial to reach the conclusion that all the specimens of this particular breed are born racers.

Redfield (1914) has proven that the results of race horse breeding can be improved upon by deviating from the method which is popular today; *i.e.*, if the specimens used for breeding purposes are not taken from those that do not run races themselves, but if actual race horses—the fastest animals—are selected for such purposes. Such a method will develop a breed of still faster horses.

In connection with this, it is interesting to notice what Redfield (1914) mentions: A horse, after attaining a certain speed, usually falls from a trot into a gallop. But it is different with a horse trained to trot. Originally, a horse had to be trained for that with difficulties, but nowadays, the foals of trotting horses are rarely to be induced into a gallop. They attain their greatest speed in trotting. Similar basic facts, with only the necessary changes, also hold true as far as the breeding of cattle and other domesticated animals is concerned.

To make a long story short: the animal (not different from a plant) acquires suitable and possible characteristics and abilities by its own strength, and by way of heredity gradually passes them on to its offspring. It is the task of the breeder to apportion to the animal those conditions and that training which, regarding the acquisition and the inheritance of the best and most desired characteristics, open the greatest possibility of development. Then, selection has only to be resorted to so that the results achieved will not only not be lost, but even intensified and broadened.

What immeasurable power the inheritance of ac-

quired characteristics, exploited systematically, gives into the hands of the breeder of animals and the horticulturist only the future will reveal. Out of a thorough understanding of the inheritance of acquired characteristics, we may expect nothing less than a revolution of the complete method of breeding animals and raising farm products. It will be necessary to change the now prevailing methods from the very bottom: then, in a much shorter time and with greater certainty of the aim desired, the result will be finally achieved.

"From the very bottom"—and this is not intended to be disheartening—this change from the very bottom will not mean so very much! Unconsciously, and quite well observed, the inheritance of acquired characteristics even at the present time plays a part in breeding. To disregard the inheritance of acquired characteristics would be impossible, because without this progressive principle of nature—I wish to repeat here—no creation and no improvement were possible. Already, now, racing makes the race horse, and milking makes the milch cow a quantity-producer. One only has to compare the scantily developed udder of an undomesticated breed of cattle with that of a breed of highly developed, domesticated cattle—the latter having a heavy udder, well rounded out, greatly enlarged, its general make-up rendering it unsuitable for the animal to live at large. This differentiation was only accelerated by selection, as was brought about by changed application of domesticated specimens.

It is necessary only to get hold of this factor, now playing an uncontrolled part in breeding, to systematically exploit it for our purpose.

"Look who"—the opponents may speak up—"is telling us all this. A man who, right in the beginning of this chapter, confessedly does not know anything about breeding of domesticated animals and who, most probably, does not know any more about the raising of crops. How could he have arrived at such conclusions from breeding frogs and salamanders and lower sea-animals? Let him remain in his own sphere of the pond and the sea, and carry on his experiments there!"

Now, to be perfectly frank, my dear reader, in spite of my lack of practical knowledge on the subject of breeding domesticated animals, have I not said quite a number of things which the thinking breeder would not hesitate to think over very carefully, things which, perhaps, are not new at all to the experienced one? If even only a part of my readers side with me in this respect, I will tell them how, in studying frogs and salamanders, I could succeed in arriving at general truths as pertains to breeding:

The intense oneness of living nature—whether highly developed or still in a stage of primitiveness—is subject only to one law. What applies to a rabbit will also principally apply to man. And all those laws which I found to govern the kingdom of lower animals, and which I confirmed by breeding experiments, surely also apply, in principle, to domesticated animals and plants.

CHAPTER XLVII

INHERITANCE AND CHILD ADOPTION

America is the land of adoption. Thousands upon thousands have adopted this country for their new home, and thousands upon thousands of cases may be counted in which children are adopted in American homes. Verily, one could say that child adoption is one of the most favorite, and at the same time most constructive, indoor sports of the American nation. In more than one respect, this is an interesting product of the great international melting pot (see Chapter XLIV).

Doubtless, in adopting a child, the benefactors burden themselves with heavy responsibilities; therefore, it may be worth while to elucidate a few points which should be taken into consideration when bringing a strange child into one's home, and dwell on those questions which one should ask regarding the antecedents of the child to be adopted.

Couldn't this child bring into the well-guarded family of his benefactor harmful dispositions and perhaps hereditary instinctive criminality, which even in an environment of a most law-abiding family would assert themselves irresistibly? Couldn't this child bring into the family of his benefactor the germs of, or the dispositions for, diseases—a heritage of his uncertain origin? And could not these dispositions, even in

spite of the most careful nursing and upbringing, become virulent?

The race-hygiene of our day knows only one agent responsible for answering the aforementioned questions; namely: Inheritance. And race-hygiene knows only one remedy to purge humanity of these undesirable hereditary tendencies, and that is: Selection. After this, that which lies dormant in the germ plasm, as a determinant, matures into a complete characteristic almost independent of the conditions of life; that is, there is no deliverance from detrimental tendencies unless the individual concerned falls victim to these tendencies himself or at least remains sterile, and in that way is prevented from passing on harmful characteristics.

To be consistent, the race-hygiene of today, facing the vital questions as enumerated above, must of necessity answer them in the affirmative when pertaining to the adoption of a child. Every adoption must be advised against where hereditary degeneration seems probable, either in the moral direction or in the direction of general health or wherever such degeneration does not appear unequivocally excluded.

The way in which adoptions are consummated even today is in utter contrast to these allegedly racial-hygienical demands. The race-hygiene of the future surely will be still less exerting, but at the same time less cruel. It will insist on other demands than the one of an ethical and eugenical "clean bill of health" regarding the origin of the child. Racial-hygiene of the future will make its judgment more dependent on the environment in which the child to be adopted will be placed, and for this reason will expect results to be

more in accordance with the future than with the past.

The enormous regenerative power of environment under favorable circumstances has been underestimated up to now by the race-hygienicians in two directions: first, regarding the individual himself and, second, with reference to his progeny. It is not true that germinal determinants are unchangeable and that an individual cannot escape his unfortunate inheritable tendencies. This is nothing more than just plain superstition which originated in the Old Testament and was elaborated in the pessimistic inheritance dramas of Henrik Ibsen and, thanks to Emile Zola's novels, unfortunately became the property of the masses.

We need only take into consideration how these tendencies developed. There is hardly a possibility that all of them lay dormant in Mother Eve's ovary; *i.e.*, that from the beginning of humanity, they already were there. Deprivation and the general social order are to blame for crowding healthily inclined individuals into a life of crime, and, in the same way, vices and diseases were acquired in unhealthy surroundings. Only now this fatal acquisition penetrated to the germ plasm to eventually corrupt, as a harmful disposition, family and race.

But whatever resulted from harmful environment, and was passed on, must be possible of elimination by changing the environment for the better. Every action has to be balanced by its exact reaction; this is a physical law which, within the realm of everything living, is surely valid. Of course, this elimination of degenerative inheritance cannot be effected all at once. Life, as well as dead matter, is subject to the law of inertia

(Chapter XL) which, as far as living beings are concerned, is usually called "inheritance."

This theory is not only the result of a juggling of thoughts, but is substantiated by experimental experiences (Chapter XXXVIII, A). For this reason, it is the more disconcerting to see these theories misunderstood and taken up in the wrong light and spirit.

When I arrived in America (1923), a number of papers claimed that I had insisted that modern race-hygiene was "on the wrong track" because it eliminates only the harmful, and does not endeavor to create the healthful (Chapter LIII). Surely, the elimination of the harmful is an aim worthy of the labor of the very best, but we should not forget that we are in the happy position to reach for a much farther goal. Moreover, many a bad trait may be eliminated, not only by prophylactic measures and by suppression, but also by changing it for the better. It would really be a pity if it were otherwise, because ever so many good tendencies always dwell right close to the bad ones. Where there is a superabundance of light, the shadows are the darkest. Should both of them go down into oblivion?

There were voices denying the possibility of permanent improvement. It was claimed* that the experiences of *centuries* were in contrast to this theory; that many *thousands* of experiments had proven that environmental changes (and this would include educational influences) did not survive in the progeny. Oh, that only one of these "thousands of experiments" had been actually *submitted* to me instead of merely

* Among others by *The Press* of Grand Rapids, Mich., Dec., 1923.

telling me about them! But such a proof could not very well be submitted, because—and in this respect I altogether concur with Redfield (1923 a, b)—it simply does not exist. It is an old maxim of logic that negative assertions cannot be proven conclusively, and doubtless, many a negative statement becomes void the very second only one affirmative fact is submitted. And in the case in question, there are many affirmative proofs.

There was an impression that, according to my mind, environment is everything, while inheritance amounts to nothing. Quite interesting was the counter-part to this opinion* where I was blamed for not valuing environment enough, and inheritance too highly. Of course, it is not surprising to see one's opinion quoted accurately only when it is used as an argument against oneself—a remark which I wish to make only in passing. I know very well that the environment which surrounds the generation of today faces the environmental residue of all the previous generations.

The complex of impressions which previous environments imprinted on the long row of generations is "inheritance." The comparative minimum of impressions engraved on the individual and expressing itself in our time is generally referred to as "environment." If there is a contrast between this environment and the environmental complex formed by former generations, we face a battle of uneven powers, which, however, is not altogether hopeless, with respect to a positive outcome. If there is not such a contrast, the influences of "inheritance" and "environ-

* Among others by *The Mirror* of Manchester, N. H., Dec., 1923.

ment" intensify each other. The incongruity of the two in favor of overpoweringly stronger influences of inheritance is only fancied, because *inheritance is nothing else than environmental influences by which former generations were affected.* The question, "Which of the powers is stronger: inheritance or environment?" is expressed altogether incorrectly, because the former and the latter are not decidedly and principally different. To incorrectly formulated questions, even Nature herself cannot give an unequivocal and true answer.

Apprehension appears to exist that, with the gaining in ground of a theory as explained here, a reaction towards inhumane and, in reference to the race, non-eugenic conditions will set in. The loss of such social achievements as the sterilization of the insane and criminals is apparently anticipated. I never fought against measures like sterilization, but I contend that such and similar procedures are not the only ones at our disposition to better the race. If all the other less drastic measures are disregarded, if selection and selection alone is considered the only means to better the race, the exact antithesis of human progress is achieved along a line which is elaborated upon with the scope of Chapter LIII. Race and class hatred will then be bred and these two will ever and again mushroom into a center of contagion, thus continually destroying the eugenic endeavors of my respected opponents and myself.

We have digressed quite far from our original theme as regards inheritance and child adoption; but it was necessary to digress in order to answer this vital question as conscientiously as possible.

Now, in drawing our conclusion, this may be said: Whatever sin society committed against the family trees of hereditary degenerates, society in its own interest should atone for by numerous adaptative graftings on a better environment. Loving care and thorough education will frequently have a hard battle combating inferior determinants which were, perhaps, rooted deeply for generations. But love's labor will not be lost, and patience will win the day! Even so-called "hopeless" cases, on their merit of engendering mutual aid alone, will eventually effect more helpful than harmful results, simply because they offer a chance to practice this mutual aid which, in the end, is the most valuable inheritance of humanity and humaneness.

CHAPTER XLVIII

ENVIRONMENT AND DEVELOPMENT

THE biologists and physicians of today are inclined to underestimate the influence of the environment upon living beings and, on the other hand, to overestimate the effects of procreation. Only internal conditions are considered responsible for the development of the embryo, while the effectiveness of external conditions is almost denied, with the physical and mental characteristics of an individual irrevocably decided from the very start. Whatever future is held in store for the embryo and child remains supposedly unessential in comparison with the potentialities already contained in the generative cells of the parents and forebears.

The main cause for the rejection of environment as a factor in development is the doubt with which the theory of the heredity of acquired characteristics is confronted. The question whether the effects of environment are hereditary is, to be sure, connected with, but essentially different from, the question whether the influences of environment affect the individual or its embryonic history at all.

That this last question must be answered unconditionally in the affirmative cannot be contended by anybody now. The amount of proofs is so large that it seems superfluous to quote them (Chapter XXXVIII).

Indubitably the quantity and quality of food, of light and air (oxygen, carbon-dioxide, humidity of the air, poisonous pollutions, such as dust, smoke, and illuminating gas), the extent and nature of exercise and daily labor, spiritual and mental impressions, make themselves powerfully felt in the life and the characteristics of the developed individual as well as in the one in the process of development. It is platitudinous to remark that the early childhood is more easily impressed by these influences than the so-called "grown-up" stage, but the final stage of development is reached only when life ends, and then and only then the possibilities for adaptability have reached the goal.

It is not quite as well known that a minimum of susceptibility to influences of the environment prevails not only at the approach of age, but also at the beginning of life. As a matter of fact the development in the body of the mother yields less to condition of the environment than after birth. The embryo is better guarded against the influences in the environment of the mother, be they advantageous or not, than the suckling or the growing child. And not only does the mother's body as a whole act shieldingly, but the embryonic coverings (chorion, placenta), in particular, act as filters to allow the just starting, most delicate process of life to take a comparatively independent course. Thus the development—especially during the first weeks—is essentially dependent on the tendencies assigned to the germ at the time of procreation.

This holds true, however, only relatively and not in the apodictic form one tries to assign to it, for the embryonic system of nutrition (the placenta and the ramifications of the maternal blood system into that

of the child) provides that the living conditions of the mother, at least her nutrition, are not immaterial to prenatal development of the child and to its later fate in life.

Perhaps one may admit these facts, but at the same time argue: What difference does it make if, for example, the offspring of an imprisoned criminal mother suffers? This development one may even consider a healthy selection by which society is cleansed of refuse.

To this the answer ought to be:

Selection does not always bring about immediate discard—in the beginning only decadence, then degeneration. In spite of the bad conditions under which they are carried and borne, many offspring of imprisoned mothers survive and all too frequently grow up to be inhabitants of penal institutions themselves. One would further have to reply: If we were to prevent the carrying, bearing, and nourishing of the child in the environment of a prison, many a tendency of hereditary criminality would be suppressed through opportune, preventive measures and many a career, otherwise inescapably that of imminent criminality, could be changed for the better.

Therefore, in the interest of race hygiene, limiting regulations are to be demanded, which would afford postponement of punishment especially for convict mothers in the second half of pregnancy—legal rules, which would forbid children to be carried, borne, and nourished in jail.

That laws of such obvious humaneness and practicability for race-hygiene do not already exist, shows the notorious harm which arises if Biology goes a one-sided way, thus forcing Medicine and Hygiene also

to travel along a single-tracked way. Thus if theorizing on life exercises a detrimental influence upon practical life, this influence will be no less detrimentally reflected from sociology back to scientific biology. It has been evidenced by Goldscheid time and again, that party politics have influenced the views of professions which are seemingly non-partisan in the extreme, including even scientists, for whom, certainly, their ethical duty of truth should indicate the strictest impartiality in political matters.

CHAPTER XLIX

INHERITANCE AND OLD AGE

FREQUENTLY it is asserted—lately by Redfield (1903, 1914, 1915, 1921) and Kendrick—that the most gifted children are offspring of oldish parents. Aristotle, Audubon, Burbank, Cuvier, Froebel, Marconi, and others are cited as proofs, because the average age of their respective fathers was 54 years when their later-on celebrated sons were born. Gen. Michael Collins, whom De Valera called the "genius of Ireland," was the youngest of eight children, and his father, who was seventy-one when Collins was born, was also the youngest of several sisters and brothers.

How do Redfield and Kendrick explain this phenomenon which they claim manifests itself regularly? This assertion of Redfield and Kendrick is entirely in opposition to the general assumption that youthful parents, and those still in the prime of life, produce the most desirable children.

The longer the life, the richer in experience. One frequently refers to the "wisdom" that comes with age, and considers this a certain advantage when compared to the frequently unbalanced rashness of youth. Now, if acquired characteristics are inheritable, the experiences gathered in the course of one's lifetime should be inheritable too. If not as such, *i.e.*, as actual

experiences, then in the form—as has often been explained in the course of this book—of inherited disposition or tendency. The longer life, equal to a life of greater experience of the progenitor, in turn passes on greater riches in experience to the progeny. To express it more accurately and fittingly such riches of experience of the parent will pass on to the child the ability to gather these same experiences quicker and easier, thus enabling the offspring to collect additionally new and valuable experiences which, in turn, will become part and parcel of the inherited knowledge.

To express it with Kendrick's own words: "Since a man must think in order to continue to think, and must think hard to be able to think harder, and since more thinking (and so, better ability to think) comes from longer exercise in thinking, and since a parent can transmit only what ability he has (not what he has had or shall have), it is logical that the older the parent (senility aside) the abler the offspring.

"Had Burbank's parent been a twenty-three-year-old one, Burbank could not have been a paragon. Had Jacob Schaeffer, Sr. (former champion in billiards), been a parent at the age of twenty years, he could not have begotten the present champion, Jacob Schaeffer, Jr., who was born after his father had undergone forty years of specialized training."

Right here, *the* main objection arises: admitted that acquired characteristics, in the form of tendencies, are hereditable, under such circumstances, would not experiences of a sad nature be passed on as well as others? Is not equanimity, prudence, and the poise of mature age the result of inhibitive experiences? Are they not, as a matter of fact, just the result of

fatigue? For "in his prime" man has already "been through the mill." Experiences, be they happy or sad, tend to make one wiser; but, at the same time, it is these experiences which sap vitality, resulting eventually in aging. Does it not seem as if, for the offspring of aged parents, youth could never be the real thing, but rather a state of being blasé and prematurely fatigued?

And admitted further that, if it is something beautiful, beneficent, and biologically possible for children to participate organically in the enlarged wealth of experiences of over-mature parents, is this advantage not outweighed by the disadvantages of the beginning process of advancing age which simultaneously are passed on to the progeny? Also the stage of the already acquired auto-intoxication of the progenitors would be passed on to the progeny, in this way accelerating the aging of the race. Under such circumstances, would not the accelerated death of an individual begotten by aged parents tend to curtail the span of life of the whole race? For all these pressing questions we will have to find answers.

Lycurgus (82 B.C.), the Spartan lawmaker, decided that a marriage could not be contracted unless the parties concerned were at least thirty years old. In our time, Francis Galton and Havelock Ellis ought to be cited as Redfield's and Kendrick's forerunners, as they also express the opinion that parenthood at the more mature stage of life is more desirable. But all of them apparently aimed only at maturity, and not over-maturity of the parents. Only in the prime of life parenthood should come into its own, but not at the time when the zenith of life has already been

passed, as is the case when man has passed the half-century mark.

The unfavorable experiences gathered in the course of time regarding the primogeniture ("the right of the first-born") does not prove the contrary. "The delusion," Kendrick asserts with admirable logic, "that the eldest son should be the crown prince has been the historical *damnum fatale* of royalty and has left the word 'king' without dignity, except in a poker game. It is now out of style to refer to kings as syphilitic, scrofulous, etc. They are merely half-baked. Notwithstanding that their training is the best that the realm affords, it cannot compensate for their congenital mediocrity, thanks to primogeniture. . . . The kings are bred just wrongly. Had they been bred by ultimogeniture for the past thousand years, they would have developed into a species of supermen."

I would not go so far as to underwrite every word of the foregoing statement. To my mind, there are two other reasons to be blamed for the débâcle of "the breeding" of aristocrats and monarchs. My first reason, not as weighty as the second, is somewhat closely related to the reasons that Kendrick holds responsible. Many sovereign and aristocratic families are, as a matter of fact, "merely half-baked," not because the respective parents lacked the necessary supermaturity, but because they were immature. To be sure that the family tree will not wither and that the throne will not be left unoccupied, haste seems to be the word, and for this reason, primogeniture encounters the exact opposite of the recommended ultimogeniture. In both these cases, the titled begetters lack the necessary propagative maturity; in the one

case they are not mature enough, and in the other case they are too much so.

More important is the second reason: inbreeding. It seems to me that it is the latter which is mainly to be held responsible for the accumulation of those symptoms of degeneration which eventually engender the physical and moral decline of the aristocracy and its flower—monarchy.

Observations within the limits of such manifestations as appear in the realm of man usually do not furnish us with sufficient material to answer our questions. How would it be, then, if we once more would resort to experiments on plants and animals, as we have done so often before in the course of this book? Unfortunately, the vegetable and animal experiments do not yield sufficient insight into the intricate connections between inheritance and old age. Not that experiments along these lines are intrinsically unsatisfactory for the solution of this problem, but because up to now too few tangible experiments have been conducted. According to verbal information by Professor H. S. Jennings of Johns Hopkins University, one of our most untiring research workers, it is to be hoped that, thanks to him, we will soon be in a position to have more exact material at our disposal. For the time being, however, we will have to be satisfied with the following results:

C. Z. Allen bases his reports on experiences in cattle breeding. He appraises the value of the progeny according to the quantity of butter-fat that one cow produces in the course of one week. Allen came to the conclusion that the age of the parental animals does

not permit a valid deduction regarding the value of the progeny.

On the other hand, E. Zederbauer (1917) infers that the appearance of race hybrids is decidedly influenced by the absolute and relative age of the parents. If a race of peas which produces yellow seed is crossed with one whose seed remains green, the inbred "grandchild" generation is subject to Mendel's so-called Rule of Segregation, *i.e.,* aside from fifty per cent of hybrids, in twenty-five per cent and twenty-five per cent, the race of the two "grandparents" is resurrected. The blending in the "children" generation makes room for separation in the "grandchildren" generation. According to Zederbauer, the numerical material cannot be depended upon to such an extent as was assumed up to now, because the quota of segregation depends to a great extent on the age and the difference in the age of the parental plants. At any rate, here we have a proof that the force of inheritance—"dominance" as Mendel called it—is influenced by age.

But would one be justified in insisting that, corresponding to the numerical deviation, the intrinsic value of the progeny has been changed? Suppose, on account of advanced age of the parent plant, the crop yields more yellow peas than was expected; does this testify that the second generation is of a higher or of a lower value? If the breeder, for one reason or the other, is desirous of obtaining yellow peas, this deviation would make the crop more valuable; but in case he was desirous of obtaining green peas, then the crop would have lost in value.

A valuation like this is relative and subjective and

was tagged on to the result only by a partisan—in this case, the raiser of the crop. The absolute, so to speak, nature-decreed value of the peas most probably has remained stationary, just as in the case of the milch cows in C. Z. Allen's experiments. Aside from certain circumstances, where a certain color of the seed could possibly be more preferable, nature seemingly does not care whether the peas remain green or change to yellow. The vitality of young plants is hardly impaired in any way by the color of the seed.

General experience, which does not need to be proved, claims that the power of propagation and the quality of the progeny diminish pro ratio to the advancing age of the progenitor. Prematurity of the parents, as regards quantity and quality of the progeny, has, perhaps, not quite as unfavorable influence on the progeny as post-maturity.

The number of the offspring in relation to their intrinsic value, in accordance with what Goldscheid and I (1918 a) stated, is proportioned inversely, *i. e.*, the more offspring, the higher the rate of death and the lower the stage of development at which they first see the light, whereas fewer offspring have a better chance to remain alive as well as to develop properly.

But there are cases when this inverted proportion changes into an equal apportioning. Reasons for this are, among others, the insufficient, or especially the more than sufficient, age of the progenitors. Even though of diminished propagative power, where, as shown before, a valuable progeny is begotten, they beget a decrepit and unsatisfactorily developed progeny. The rejuvenation which, in living beings who propagate themselves bi-sexually, manifests itself in the

germ plasm and in the offspring is the more imperfect the older the parents are. The child of aged parents is, in a way, already approximately in just as aged a condition, so far as his vitality and his later-to-be-developed power of propagation are concerned (Kammerer, 1918). Together with the other inborn and acquired characteristics of his parents, he also acquired the parents' aged condition. One should not forget that duration of life and age are hereditary characteristics, too! If ever so young as regards his actual age in days, months, or years, the belated offspring dwells relatively close to the threshold of old age.

In consideration of all this, other arguments are silenced. To the questions raised at the beginning of this chapter, we will therefore have to make the following reply:

We have to differentiate exactly between the inheritance of special characteristics and the inheritance of general vitality. According to all that we know about the inheritance of acquired characteristics, it is to be assumed that experiences gathered in the course of life are actually (as tendencies) passed on to the progeny. Looking at things from this point of view, late marriages and late propagation seem desirable. But we cannot expect that offspring begotten late are endowed with sufficient vitality to be able to exploit the inherited riches of experience thoroughly in the future. The acquired characteristics (in this case experiences) would, in spite of being an advantage to the race, soon become lost again on account of the weaklings eliminated by natural selection.

Here we once more face one of those cases (com-

pare with a second case as related in the Chapter LII) where a law is not exactly invalid, but where it is actually rendered unworkable by circumstances. There is an inheritance of experience in accordance with the natural law that acquired characteristics are inheritable, but the general physical deficiency of those who are "born too old" is an obstacle to the clear and useful manifestation of inherited experiences.

For all the reasons enumerated in the course of this chapter, it will be better for us to renounce enriching of our heritage by late marriages. We may do this the better, because those individuals most fervently desired in the interest of the race, and those endowed with extraordinary physical and mental characteristics, are mostly surprisingly premature in these respects. Wilhelm Ostwald, in his book, *Grosse Männer, Studien zur Biologie des Genies,* records that the most gifted mentally are completely ripe between the age of twenty and thirty. Whatever they produce after their thirtieth year is no longer the result of new thoughts, but rather the "building up," an elaboration of former thoughts which, as thought-germs, manifested themselves in early youth.

Now, inheritance is effected by way of the germ plasm and the tendencies and determinants hidden there. As far as the inheritance is concerned, it may be of equal importance whether the ability for mental creation is transmitted by way of the germ plasm, or whether already consummated creations have to retrogress into germs in order to make possible the passing on of such characteristics from one generation to the other.

After all, what do the experiences of one individ-

ual amount to compared to the superabundance of experiences acquired by former generations? Those especially who believe in the inheritance of acquired characteristics will notice the incongruity between inheritance and experience.

And those who, in inheritance, conceive nothing else but the after-effect of experiences which our forebears lived through, will notice an overwhelming abundance of these former experiences, in contrast to those which we are able to gather in our days.

Of course, the single individual does amount to something. If the time in which we are able to gather experiences in each and every generation can be prolonged for only a few years, it will, in the end, amount to an enormous gain. And we should not forget that quite a number of things, which live in us as the experience of generations gone by, have already survived themselves. We are in need of a constant revision of acquired experiences. And the more of these acquired experiences that are passed on, mentally and physically, to our progeny, the more the latter will be in a position always to be "up-to-date."

But how does this help us if the enriching of experiences, the prolongation of the period when experiences may be gathered, has to be paid for with vitality? However, it is not so very difficult for us human beings to find a way out of the dilemma. We are the proud possessors of an enormous hoard of experiences—verbal traditions and those passed on in writing—outside of our brain and our germ plasm. Exactly as in organic transmission, this exterior, non-organic heritage of the dark past is continually enlarged and revised in our days. There is *another* thing

analogous to our innermost heritage: gradually it assumes a stage of germinative tendency, a form freed from superfluous elaboration which is easier to pass on and to enlarge.

In this respect, to absorb this traditional heritage and to keep on building upon this foundation, a youthful human being is doubtless much more efficient than an aged one. The advantage of the latter, on account of personally acquired experiences, is balanced by the more extended possibility to absorb foreign experiences in condensed form by the former. In a certain sense, a young man who has learned much is more experienced than an old one who has only lived longer.

Both types of experiences, however (tradition as well as actual experience), become embodied in the germ plasm. Even the exterior heritage which is passed on to us by word of mouth and by writings, rounded out and shorn of superfluous additions, becomes our interior heritage. But how do those two compare with each other as to their value to the succeeding generation? Eugenically speaking, who is more at an advantage regarding progressive development: the old, self-made man, or the young man who within himself carries and exploits an extract of uncounted self-made men? It may be that the absorption of foreign experiences is not a very propitious method, and that personally acquired experiences can never be replaced by anything else. But, in spite of this, our answer can leave no doubt: to marry early is the lesser evil.

CHAPTER L

INHERITANCE AND REJUVENATION

Do not the almost miraculous discoveries of our days offer a second way out of the dilemma of being unable to exploit eugenically the wealth of experience that goes with advanced age, because it develops simultaneously with diminished vitality? Does not the achievement of artificial rejuvenation offer a greater possibility to fulfill the demands of Redfield (1903 etc., 1915) and Kendrick to replace "primogeniture" by "postgeniture"?

The disadvantage of experiences acquired by tradition, in comparison to those acquired personally, has already been mentioned. It is almost as if somebody should try to acquire training and proficiency in athletics simply by looking at Olympic games, instead of subjecting his own muscles to intensive training. Does it not constitute a *testimonium paupertatis* for anybody who comes to accept the inheritance of acquired characteristics? Does it not even amount to a débâcle of this theory, if exterior tradition is called upon instead of inducing and depending on the inheritance of experiences?

Now, it is not quite as bad as all that, because, as mentioned before, in accordance with the teaching that acquired characteristics can be passed on, it is necessary that not only personally acquired, but also transmitted and accepted experiences shall be inheritable.

The child of educated parents will be able to acquire knowledge easier than the offspring of analphabetes. But, just as transmitted experiences do not engrave themselves as markedly into the memory of the individual as experiences acquired personally—being less vivid and of less value—so transmitted experiences, in the inherited memory of the race (*mneme*), lose even more in distinctness in comparison with personally acquired experiences, and become secondary to the latter. Tradition is just patchwork and no genuine substitute, and very often has to be discarded if real progress is to be made.

Added to this is another disadvantage of experiences transmitted by tradition; namely: if the hereditary effect manifests itself less strongly than the effect of personally acquired experiences, the real reason most probably is that these traditionally transmitted experiences do not succeed in noticeably extending our brain. The immense progress of science and technic, made since the classical ages, about the time of the great Greek philosophers, was not accompanied by an equally enlarged and refined improvement of the human brain. Once in a while, this inharmony is resorted to in order to deny the inheritance of acquired characteristics as a whole, even though this incongruity is not as marked as is usually assumed. J. Petzoldt, especially, has tried hard to prove a progressive development of the human brain within the last two thousand years, endeavoring thereby to salvage the inheritance of acquired characteristics. But, doubtless, there is a decided incongruity between the progress we have made mentally and the progress the human race made organically, and this incongruity is the result of

our depending too much on tradition and too little on our own acquisition. Without organic development and adjustment, all progress must, of necessity, come to a standstill. Is it not easy to notice the admonishing signs (J. H. Kellogg, 1921; Wiggam) of such a halt all round us?

The capacity of our brain is limited. Consciousness is unable to absorb again and again all the traditions of posterity and to add to it, if the organic foundation is not correspondingly enlarged. The capacity of the theater where the spectators (the nerve-cells) watch what is happening on the stage of life has simply to be enlarged. This can be achieved, if not exclusively, surely to a certain extent, by nothing else but personal experiences. That is to say, that the spectators do not remain spectators only, but also become actors on those boards that symbolize the world, or at least life.

Let us now return to the question with which we began this chapter: Does not the discovery of methods of *rejuvenation* offer the possibility of combining the wealth of personal experiences of advanced age with the physical strength of a young body? In the previous chapter, we ascertained the fact that years are not a deciding factor as regards the physical condition of an organism. A "young" person may be very old, his condition and his aim in life considered. This undependability of time, in comparsion with the length of life, pertains not only to the beginning, but also to the concluding chapters of life. An "old" person, an individual who has seen many a summer, may feel young in spirit and may be quite efficient in every respect. Under such circumstances, do our eugenical

objections, regarding the inferior quality of a progeny begotten at an advanced age, still remain valid?

One thing is certain: The man of forty, biologically speaking, is always older than he was in his twenties, even though there may be men in their twenties who, biologically, are older than the man of forty. It is impossible, however, to draw a sharply distinguishing line between youth and old age. It is equally impossible to simply disregard or, as Kendrick puts it, set aside senility, when he recommends propagation at advanced age, provided that both parents are not senile. In some respects they surely are, because old age approaches gradually and hardly noticeably; aging starts right at birth. The more individual experiences, the less the strength of youth, because experiences themselves "use us up" and make us old. Let me repeat once more: Even though the experienced man in his forties may feel like a youth, in spite of his feelings, he is nearer to old age than he was only a few years before, and for this reason, in his capacity as a sexual being, he cannot be as highly valued any more as he was in his period of storm and stress.

One's individual conception of considering oneself young or old is just as little a "yardstick" to measure one's age biologically, as is the span of life a living being lived through a criterion of the condition of his tissue. I hope that physico-chemical methods, just now worked out by Ruzicka (Prague), will finally make it possible to ascertain "absolute age", *i.e.*, the biological condition of the living tissue aside from the length of time this tissue was part of a living body. The tissue of rats, rejuvenated by Steinach (1920),

when subjected to the physico-chemical tests by Ruzicka (1922) and his collaborators, reacted in identically the same manner as did the tissue of young rats, whereas the tissue of old non-rejuvenated rats, time and again, reacted decidedly differently at the test.

Propagation at a mature age, after a life crowded with valuable experiences—verily, this would be an ideal realization if, in spite of the number of years lived through, youthful strength still makes this possible. To realize this ideal, within certain limits, seems to be possible now by taking recourse to modern methods of rejuvenation. Experiments on animals, which have always everywhere given us the foundation for understanding and improvement, for knowledge and its application, permit us to raise our hopes even as high as that ideal.

It has been ascertained that the progeny of rejuvenated animals which were formerly senile (males as well as females) could not, in any way, be differentiated from the progeny of genuinely young animals. Neither in their own development nor in their power of propagation did they manifest themselves as belonging to formerly senile parents. Steinach (1920) reports that the litter of old female rats, which had been sterile long before they were subjected to rejuvenation, "developed especially well."

Valuable bulls (Otmar Wilhelm) and stallions (Lengeman), thanks to a rejuvenation operation—which, especially on the male, is very easily performed—could still be utilized for breeding purposes beyond the usual age limit. The Steinach Method of Rejuvenation which consists in a ligation (vasoligature), followed by a severing (vasectomy) of the spermatic duct,

can, of course, be applied only to one of the two spermatic ducts, in the event that (aside from sexual ability) the propagative ability is to be conserved, restored, and prolonged. More on this theme may be found in my book on *The Prolongation of Human Efficiency* (1923 b).

Mentioning the Steinach Method gives me occasion to dwell on another angle, combining rejuvenation with inheritance of acquired characteristics.

The sexual glands consist, aside from the connective tissue, of two main tissues: first, the generative tissue which, in certain canals and follicles, generates the germ cells (eggs in the female, and spermatozoa in the male); and second, the interstitial tissue. The generative tissue, or the generative gland proper, takes care of the exterior secretion (*excretion*). The interstitial tissue, however, called the "puberty gland" by Steinach, takes care of the inner secretion (*incretion*). The fluids prepared in this tissue mix uninterruptedly with the blood circulation, gaining a strong chemical influence not only in the development of sex differentiations, but also in the preservation of general vitality.

Interference with the spermatic duct results in compensatory processes in the neighboring sex gland. To express it morphologically: the result consists of a retrodevelopment of the generative gland proper which, for a certain length of time, may even deteriorate. At the expense of this deterioration or retrodevelopment, the interstitial gland proliferates; to express it physiologically: the excretion, for the time being, is surpassed, thus gradually stimulating the incretion of those substances (*"hormones"*) which are

responsible for a heightening and preserving of vitality and joy of life.

The same readjustment between generative and interstitial tissue can also be effected by other measures than a disruption between the generative gland and its outlet. Similar tissue changes appear in the wake of every mild irritation which is just sufficient to put the more sensitive generative tissue out of commission, at least temporarily, thus allowing the more hardy interstitial tissue always to be "a few jumps ahead."

Poisonings of an organic or non-organic nature, infections brought about by bacteria, deprivation, a congenital false position of the sex gland, experimental transplantations of sex glands, heat, exposure to radium, or irritations effected by rays like the X-ray and ultraviolet rays, result to a more or less decided degree in the puberty gland proliferating at the expense of the generative gland proper. Graded accordingly, all influences enumerated so far (even those brought about by diseased conditions not excluded) must produce rejuvenative results, as long as these influences do not seriously damage the whole organism.

In connection with this, it is especially worth noticing that tissue changes of the sex glands manifest themselves again in the progeny, even though the latter grew up under absolutely normal conditions and without being interfered with in the slightest degree. This hereditary result, in reference to X-rays, has been proven by Manfred Fränkel. As regards heat, Steinach and myself (1920) succeeded in principally establishing this fact. To avoid misunderstandings, I wish to repeat right here that the interference with

the generative tissue does not have to be a serious one or of long duration. Animals, in which the puberty glands were especially well developed, remained fertile just the same or became fertile again after some time.

The fact that the result of heat could manifestly be inherited was first discovered in the following way: On rats kept at a temperature of 77 to 104° F., an enlargement of the testicles was observed, which was chiefly the result of a proliferation of the interstitial tissue cells. A few of these testicles were prepared and forwarded to Dr. Gustav Fritsch, Professor at the Anatomical Institute of the University of Berlin, for the purpose of checking up our findings. Fritsch confirmed the fact and, at the same time, noticed that the proliferation of the interstitial tissue in some of the preparations was much more marked than in others. Referring to the labels attached to the different preparations, it was easy enough to ascertain the origin of the more and of the less changed testicles. It then became clear that the less changed preparations were those of the first, while the more changed preparations were those of the third generation bred and kept at a high temperature.

Simultaneously with the changes in the tissue of their sex glands, changes in the entire appearance of each animal are to be observed. Many of the exterior parts of the body show a stimulated growth, even though the body of rats kept at a high temperature compared to rats kept at a low temperature, as a whole, is noticeably reduced in size. The rats kept at a high temperature have a more pointed mouth, bigger ears, and longer, narrower paws. As men-

tioned before, they have a decidedly enlarged scrotum and, finally, very noticeably long tails. During the cold season, they do not change to a heavier fur, but the smooth, somewhat scanty growth of thin hair of their summer fur remains permanently. Findings analogous to those of H. Przibram (1910 a, b; 1923 b) on white rats, were effected by F. Sumner on white mice. Restored to an intermediate temperature, these rats and mice hereditarily retained the changed characteristics acquired at a high temperature. Following up these phenomena of inheritance, especially with reference to Przibram's experiments on rats (1923 b), a number of intensifications and contrasting phenomena were observed, the importance of which can only be surmised as yet.

In principle, agreeing to the foregoing, M. Fränkel reports that guinea pigs, subjected to X-rays, developed a dwarfed form which became hereditary. He furthermore reports that such spots where only the parents or even the grandparents were influenced by X-rays, had become bald. In reference to the *theory* of the inheritance of acquired characteristics, the results enumerated here are not so favorable, because we are dealing here with changes in the sex glands. Running parallel to exteriorly noticeable changes to a certain extent, they are perhaps only indirectly induced by them. For this reason, the objection (already mentioned in Chapter V) that a direct influence on the germ cells by heat and exposure to rays had been established, seemed especially substantiated.

In connection with thermometrical findings by Przibram and some of his pupils (Congdon, Bierens de Haan, and others), it was already mentioned in

Chapters V and XIII that even warm-blooded animals, to which class rats and guinea pigs belong, do not always have an unchangeable blood temperature, but that their body temperature fluctuates, especially when still sucklings, and in accordance with the temperature of the environment, even though this fluctuation may amount to hardly one or two degrees. However, this fluctuation is sufficient to induce exterior and interior variations. Bierens de Haan and Przibram (1922) ascertained that by applying drugs to the experimental rats, in order to stimulate febrile conditions, he succeeded in breeding a form thinly covered with hair and with long tails. This shows that it is unnecessary to raise the temperature of the environment, and that it suffices to raise the body temperature of the animal to achieve an analogous result. Or, to express it even plainer, if animals are subject to high temperature, the heat of the environment at first results in a continuous rise of the body temperature, the latter inducing all the other changes.

Whether these changes are passed on by genuine inheritance or by after-effect (that is, by direct influence on the germ cells) does not matter, as far as the *practical* side of inheritance and rejuvenation is concerned. The mere fact that they are actually passed on is of the utmost importance. From this, we may deduce that it may be possible to bring rejuvenation effects to bear upon succeeding generations. The rejuvenation treatment results in the proliferation of the interstitial tissue of the sex gland. An individual subjected to this treatment does not lose his propagative powers but regains them, provided that only one of the two spermatic ducts has been ligated. The rejuvenative proliferation of the interstitial tissue re-

curs, even without being stimulated in any way in the progeny, and in one way or another is "hereditary."

From this there follows that males (young as well as old) whose youth and general vitality were intensified by a unilateral vasoligature (the ligation of only one spermatic duct) will produce a progeny strengthened in the same way right from the start. The same holds true for females whose ovaries have been stimulated by X-rays. Of course, this possibility of *hereditary rejuvenation* is still in need of thorough experimental investigations. But in the light of the foregoing Steinach's observation (1920), that the progeny of rejuvenated males and females were especially well developed, assumes distinct importance.

This hint at "hereditary rejuvenation" and the intensification of efficiency by strengthening the puberty glands of generations is just meant as a promising outlook and nothing more. As long as the demanded and necessary investigation of this new problem has not proved that we are not deluding ourselves with false hopes, we must renounce the right to deduct eugenical measures from it. Up to now, rejuvenation treatments are not generally enough applied to furnish the possibility of laying down such eugenical measures with any great amount of certainty. For this reason, for the time being, we still have to figure on the natural cycles of aging and propagation. We have to be satisfied with that rejuvenation which, as mentioned in the foregoing chapter, is effected by the process of propagation *per se* on the product of propagation, the young living being of a new generation. This natural rejuvenation is surely the more distinct, the less the progenitors themselves were in need of artificial rejuvenation.

CHAPTER LI

INHERITANCE AND DUCTLESS GLANDS

THE rejuvenation of the whole organism is preceded by a revivification of the ductless glands, that is, the glands with an inner secretion (incretion). In the foregoing chapter it was explained that at present only one of these ductless glands is approached by way of an operation—the sex gland (gonad). But, as all the ductless glands closely and precisely coöperate with each other, the revivifying influence resulting from the rejuvenated sex gland by way of blood circulation is, in turn, passed on to all the other ductless glands.

If, for rejuvenation purposes, it were possible to resort to some other ductless gland aside from the sex gland which is exclusively utilized at present, the revivification of the whole organism would start from this other gland, simply proceeding in a different rotation and manifesting different primary symptoms. This assumption, first voiced by myself and later adopted by Leschke and Koppányi (1921), has already been substantiated by Buxbaum, who took recourse to implanting a thymus gland, and by Romeis (1922), who availed himself of a youthful liver. Only lately, R. G. Hoskins declared that, to his mind, the thyroid and the pituitary glands seem to be even more important as "organs for rejuvenation" than the sex gland. These same ductless glands, which regulate

the growth and well-being of the whole body, doubtless play an important part in reference to inheritance.

In Chapter I it has already been explained that the chromosomes of the germ cells are to be considered as the vehicles of inheritance which, in the form of certain substances, hide within and carry with themselves hereditary tendencies. In the course of the growth of the body by division of cells—a multiplication of the original germ cell—these hereditary tendencies are distributed to the respective parts of the organism. If we consider the chromosomes as the *vehicles* of heredity, the ductless glands have to be looked upon as the *executives* of heredity, with the former offering the diversified tendencies and the latter applying them suitably. The ductless glands are in full control of this division of cells, and are able to stimulate this process on account of the hormone produced by them, and are also in a position to retard the growth of the body by other substances. By stimulation and retardation, the ductless glands (for this reason, also called "growth glands") take care of suitable distribution and exploitation of hereditary tendencies, that is, of the application of the hereditary substances together with the continually multiplying germ cell nuclei to those parts of the body where they are needed. Only in this way, in due time, may hereditary tendencies develop into what they were meant for.

The "Localization" of hereditary determinants which up to now was missing from all hypotheses on inheritance, manifests itself in the reciprocal effect between germ plasm and inner secretion.

As the term, and the problem of, organic "inheritance" is usually applied analogous to a heritage in

earthly riches, it seems quite proper to continue to use this analogy in the interest of better understanding. The chromosomes may be compared to a savings bank, where inherited money is deposited to bear interest. The ductless glands may be compared to the clients of this savings bank, who put their savings into circulation and eventually enlarge them. Applied to "organic" capital, this would amount to newly acquired characteristics.

As a matter of fact, it seems quite probable that all, or at least most, of those exterior influences, which we watched at work in the course of the biological chapters of this book, avail themselves of the ductless gland system to enrich our capital of inherited characteristics by new acquisitions. It has frequently been mentioned that a direct influencing of the germ cells usually is not sufficient to effect genuine heredity of acquired characteristics; rather, it is necessary that the strata of the body, between the germ and the changed part of the body, play the rôle of mediators. This mediation, essentially or even exclusively, seems to be the task of the ductless glands. Without the coöperation of the ductless glands, newly acquired characteristics could not become part of the body of the directly influenced individual nor could they become part of his germ plasm.

This probability seems best substantiated in reference to the vertebrates (mammals, birds, reptiles, amphibia, fish) which possess a highly developed system of ductless glands. It is here, where probability has almost become certainty, that the inner secretions (hormones) of the ductless glands are the executive organs of germ-plasmic determinants. Here stimu-

lating and there retarding, the ductless glands (provided the mixture is the right one) take care of suitable apportioning, of synchronized action of all the members of the body, of beauty and general efficiency.

To be sure, inner secretion is not a characteristic of the vertebrates only, but is common to all living beings. The ductless gland of a vertebrate (for example, the thyroid or adrenal) is nothing but a special organ for inner secretion, analogous to lungs or gills, which are special organs for respiration; to the intestine, which is a special organ for digestion; or to the eye, which is the special organ for the perception of light. But there are other organs and tissues which breathe, digest, are sensitive to light, etc. And they also have an inner secretion. Every tissue produces inner secretions, because every tissue has its own individual metabolism whose products are different from those of other tissues. Blood, lymph, or vegetable fluids, passing through a tissue, undergo chemical changes, because they absorb the secretions of the tissue they penetrated. But this circulating fluid not only absorbs; it also exudes secretions. Changed itself, the circulating fluid naturally exudes a changed secretion which, in turn, again works a chemically changed influence. With the assistance of these fluids, circulating through the vessels of the body, chemical changes are transmitted to different parts of the organism and we come to the conclusion that, by the secretion of every tissue and every cell contained in it, all the other tissues and cells of the body are influenced to a greater or to a lesser degree.

From this it follows that any part of the body, affected by exterior stimuli, imparts an intensified secre-

tion to the interior of the body; and that each and every part of the body, changed by exterior influence, exudes a changed inner secretion. The intensified and changed inner secretion, in turn, imparts to all the other regions of the body, including the germ cells, something of the received stimulation.

Where inner secretoric organs (ductless glands) are a part of the organism, as in vertebrates, the reciprocal effect will be especially prompt and noticeable. The inner secretion can be observed not only in vertebrates, but in all animals and plants. It even holds good for the protista, which, in the nucleus of their one and only cell, possess a tool which combines the faculty of germ plasm with that of inner secretion and, for this reason, unites in itself the substances of propagation as well as of stimulation.

How exterior stimuli direct the hereditary substances—first, in affecting the individual, then hereditarily his progeny, by mediation of the ductless glands—will now be made clear by an example of biological research.

Many animals do not develop directly; emerging from the egg, they do not look as they will look when completely developed. Between the time of emerging from the egg and the final development, intermediate, or larval, stages are manifested. An example of this is the butterfly developing from the caterpillar, or the frog developing gradually from the tadpole. It often happens that those immature stages are retained overlong, sometimes even omitting the metamorphosis into the final stage. Such manifestations are called *neoteny,* that is, the retaining of the youthful, immature stage.

It is very remarkable that *neoteny* only interferes with developing into the final stage, but does not block growth or maturing to propagative power; *i.e.*, complete *neoteny* checks the differentiation, but not the gain in size, and just as little the full development of sexual powers. Totally *neotenic* specimens may propagate themselves and may even, under certain circumstances, pass on their development checking anomaly.

That this is a rule was first observed in the Axolotl *(Amblystoma mexicanum)* which is found in the lakes of Mexico. This animal lives there for the duration of its life, retaining the gills of the young state through life and apparently never, or only in very rare cases, metamorphoses into a full-fledged newt, breathing through lungs. Taking recourse to all kinds of artificial means, Marie von Chauvin succeeded in forcing the metamorphosis of the Axolotl. The main methods employed consisted in keeping the experimental specimens in water so shallow that, as often as the animals moved, their heads stuck out of the water. The bottom of the aquarium was slanted upward, to make it easier for the animals to leave the water entirely. The water had been boiled beforehand, so that the animals did not get any air through their gills and were forced to use their lungs for respiration. These methods, obviously, resulted in great mortality among the experimental specimens.

Those specimens, however, which successfully metamorphosed into the terestrial form begot a second generation which underwent the changes without being forced, provided that at the moment when the animals were just ready to metamorphose a possibility was offered them to leave the water. This shows that,

in the Axolotl, the larval or aquatic form is the most frequent and *neoteny,* for this reason, almost its normal condition. It was not so great a mastake as all that, before the terrestrial form of the Axolotl was known, this animal was classified as a newt with permanent gills (to which group also belongs the newt Proteus) and was called *Siredon pisciforme.* But once the transformation of the larval form into the terrestrial form is set going (which in the beginning was extremely difficult) the tendency toward complete development is passed on to the progeny of the metamorphosed specimens.

The Axolotl, since the days when Marie von Chauvin succeeded with her experiments, has continually been bred in Europe, but always in its larval form. If anybody were to repeat the experiments of Marie von Chauvin today, almost insurmountable difficulties would face him, because, due to the permanent breeding of sexually mature larvæ, the urge to metamorphose has almost been completely lost. Now the retaining of the immature form (the *neoteny*) has been hereditarily fixed to almost complete unchangeability. This, at least, holds good in reference to the exterior influences applied to Marie von Chauvin's experiments: shallow water which, in addition, was air-poor. Through the experiments of C. O. Jensen, Kaufman and Laufberger, however, we have since then come to know a different means by which even the most stubborn Axolotl larvæ, at any stage of development, may be induced to metamorphose. This very effective method consists in feeding or administering in some other way to the specimens fragments of thyroid glands. Thyroids used for this purpose may be de-

rived from any kind of animal (including domesticated mammals whose thyroid glands are obtainable from any stockyard) because the inner secretoric substances are not at all of specific differences, even if derived from different kinds of animals.

The successful experiment to induce an animal, so little inclined to metamorphose as the Mexican Axolotl, to be ready to change its form, as the result of being fed with an abundance of thyroid gland, is very instructively supplemented with experiments on other amphibia larvæ. In the common triton, salamander and frog larvæ, the inclination to change normally is so decided that, even when living in deep water and when prevented from coming to the surface of the water, it is impossible to forestall the reduction of their gills and fins hemming the tail or the development of their lungs or anything else which belongs in the complete metamorphosis of the aquatic form. Such animals drown if not placed on land in time. But this irresistible inclination to metamorphose immediately ceases as soon as these frog and newt larvæ are robbed of their own thyroids. *Vice versa,* these larvæ, fed with pieces of thyroid, may be induced to change into very little frogs and newts at an abnormally early stage.

Therefore, the thyroid gland is certainly one of the inner secretoric organs which rules the metamorphosis of the amphibia. The quantity of the available thyroid hormone is in direct ratio to the inclination to metamorphose and in inverse ratio to the length of time which is necessary for metamorphosis. But the thyroid gland is by no means the only organ of this kind which

stimulates or retards, induces and accomplishes the metamorphosis.

If tadpoles are fed with thymus glands, their transformation is retarded or even checked. The extracting of the thymus gland, on the other hand, stimulates the thyroid gland to greater action, and so causes the sex characters to develop especially large. The extirpation of the pituitary gland results in gigantic larvæ which do not metamorphose at all. This result is most probably brought about indirectly by a simultaneous atrophy of the thyroid. With their pineal glands removed, tadpoles grow faster and change early, but not completely. Experiments of this kind, in connection with the influence of inner secretion on metamorphoses, have already been conducted by a great number of scientists, among them Leo Adler, Babak, Gudernatsch, Hanko, M. A. van Herverden, C. O. Jensen, L. Kaufman, Laufberger, Novikoff, Romeis (1913-1918), Swingle and Uhlenhuth (1923). Very important new findings are just now resulting from experiments carried on by Hogben in Crew's Department for Animal Breeding at the University of Edinburgh, Scotland, and in Harrison's Zoölogical Laboratory at Yale University, New Haven, Conn., as I had occasion to witness.

The rules referred to seem established beyond doubt. If we were cognizant of nothing but these facts they would permit us to deduce that the metamorphosis (and for this reason, the condition of development at the time of sexual maturity and, with that, the whole appearance of the genus, whether larval form or a higher developed form) originated from inner stimuli

—the whole ductless gland mechanism doubtless belonging to the inner world of the organism.

But how, then, could Marie von Chauvin, by applying exterior stimuli, succeed in breaking down the resistance of the Axolotl to metamorphose and even effect the newly awakened inclination toward a change that would become hereditary? Powers, conducting a number of extraordinarily varied experiments, ascertained that exterior stimuli, for which in the last end he held food responsible, played the deciding part in reference to the metamorphosis of the Axolotl. Another experience, only different from the foregoing experiment in so far as it was not the intention to effect a transformation in an animal which usually does not metamorphose, but which incidentally resulted in an acceleration of a regularly observed metamorphosis, is the following:

The bull frog (*Rana catesbyana*) of North America takes two to three years to develop from a tadpole into a full grown frog. Because his muscular legs are appreciated as a delicacy, the bull frog was imported by the Japanese. It soon became apparent that the tadpoles, in the breeding ponds of Japan, required only four months to metamorphose, and for this reason do not even pass one winter in the larval state.

Here I should also mention my own experiments (1909, 1910 g) conducted with the Midwife Toad (*Alytes obstetricans*), not with the intention to force or accelerate, but rather to retard or even to completely check a metamorphosis. Gradually, I ascertained that darkness, cold, and an abundance of air in the water, rich food after having been poorly nourished and premature emerging of the embryo from the egg,

were conditions which tended to retard the transformation of the toad. With each one of these exterior influences, I managed to obtain larvæ which did not metamorphose in time, and already in their larval state grew to a remarkable size; but finally—and this is the most important—previous to the beginning of their first propagation period, developed into complete toads. Progeny of such belatedly transformed toads metamorphosed at a normal time. They therefore had not been hereditarily influenced by the deviating process of development of their progenitors.

A combination of all the mentioned retarding influences was necessary in order to breed a sexually mature toad larva. The progeny, although bred by artificially fertilizing a single female larva with the sperms of a normal, fully developed male, did not develop, for years and years, beyond the stage where they had only hind legs, nor did they manifest the least inclination to metamorphose.

Now, do all these experiments, which show to what extent exterior influences (as temperature, light, food, amount of water, and the air contained in it) affect metamorphosis, contradict those experiments which proved the great part of inner, and especially inner secretoric, influences? It very frequently happens that such conclusions are drawn—especially anyone who discovers an inner influence comes to the conclusion that, by his discovery, the ineffectiveness of exterior influences is proven. In a case of metamorphosis, both influences (inner and exterior) are proven in many instances and, to my mind, there is hardly any hindrance for the two groups of experiments to supplement each other.

The exterior influences first stimulate the ductless glands, inducing a secretion which is changed in quantity as well as quality. Then, the rest of the body reacts. Wherever the hand of the experimenter directly touches the glandular system, other "exterior" influences can be dispensed with. What has been ascertained here regarding the problem of metamorphosis most probably also applies to the entire problem of variation. In reference to the variation of such living beings as have no special organs for inner secretions, the general inner secretions of the tissues fulfill this task.

As we observed how the more indirect exterior, as well as the direct interior, influences brought about hereditary results, we may conclude that the inheritance of acquired characteristics is closely connected with inner secretoric processes. In the beginning of the chapter, we referred to the inner secretoric organs as "executives of inheritance." But the ductless glands are not only that (because they make it possible for the germ material to reach the proper spot, that is, stimulate tendencies into full characteristics); they are also to be considered "executives of inheritance," because it is they that mediate between body and germ plasm, between the world without and the world within.

May the analysis, so completely followed through in reference to the metamorphosis of amphibia, be a warning to us that we never should decide one-sidedly in favor of either exterior or inner influences! Inseparably, the two work together to effect changes and the inheritance of these changes. The action of exterior influences on the living matter assumes the ability of the latter for reaction. That not enough attention

was paid to this essentiality has led to numberless misunderstandings, thus damaging the science of transformation of species to an incalculable degree.

The causative connection between inner secretion and inheritance now leads us to the last point, which will justify the placing of this up-to-now purely biological chapter within the eugenical part of this book. The rational exploitation of the laws of inheritance for the benefit of higher development of the human race seems still far off in the distance. And everything that we may say about this, especially in the last chapter of the book, sounds somewhat utopian, or at least far fetched. The inheritance of acquired characteristics is not as immediately and directly applicable to human welfare and eugenics as are gland operations. For this reason, the science of the inner secretion (for example: their latest development, Rejuvenation) is enjoying a wider and more intensive interest than is the science of inheritance.

But if it becomes evident that the ductless glands are intimately related to inheritance—and to the inheritance of adaptation also—the situation changes decidedly. Then, the science of inheritance ranks with those sciences which are directly applicable. Therapy and hygiene of the ductless gland system are at present making enormous progress. Race-hygienical exploitation of this branch of biology, this "no man's land" between biology and medicine, is already becoming evident, in spite of the fact that this branch is much younger than the science of inheritance and eugenics. The sterilization of inferiors, together with its favorable effect on metabolism and the psyche of the sterilized individual, constitutes an example of race-hygieni-

cal application which we cannot very well overlook (see Chapter LIII). The improvement of the fluids circulating through our body by modern gland- or organotherapy most probably will also favorably influence the progeny of those individuals who are benefited by it directly. Mothers whose ability to nurse their babies is stimulated by X-ray treatment of the ovaries (and the ovary is a gland with inner secretion) may very well produce an identical after-effect in their daughters, just as the habit of having one's offspring nurtured by a wet nurse or "raised on the bottle" results in an hereditary deterioration of the ability to nurse one's own babies.

We are at the threshold of possibilities each one of which should be sufficient to make us forget all national and personal hatreds. If we do not deliver ourselves up to such nationalistic or individualistic peccadillos, we will be able to fulfill these new and great tasks waiting for us.

CHAPTER LII

INHERITANCE AND GENIUS

THERE is hardly a question more frequently put to the biologist than that regarding the origin and the perpetuation of great talents. And there is scarcely any other manifestation within the realm of mental achievements which bears so markedly the attributes of the miraculous as the sudden appearance and traceless disappearance of genius. Frequently begotten by parents apparently not endowed with more than the average gifts, frequently themselves sterile or progenitors of a comparatively inferior progeny, genius asserts itself in "splendid isolation."

This mode of speech is very often applied in connection with the way the world treats a genius. But the same figure of speech could also be applied to the manner in which genius, in the course of generations, puts in its appearance. Splendidly isolated, genius flares up in our world, not unlike a meteor which, after running its course, is extinguished. Neither inherited nor able to be passed on to one's progeny, genius verily seems to be a gift of the gods, which cannot be explained on a natural basis.

But where genius comes from and where it goes to seems only supernatural in an epoch which, like ours, still clings to the last remnants of a ubiquitous belief

in miracles, in an epoch which, even within the realms of exact sciences, has not entirely renounced superstition, and is still deluded into a blind adoration of authority. Simply because it is the fashion, one carefully avoids, in science, even admitting the reciprocal effect between education and inheritance. One denies that environment and education leave hereditary impressions. For reasons too many to enumerate here and which were dwelt on in detail in the Preface, one refuses to concede that, by environment and education, in the course of generations, the influence of one's forebears can be substituted. There exists hardly a problem of general and human biology whose solution is not hampered by the dogma that environment and inheritance have nothing to do with each other.

The "natural history of genius" is immediately removed into a more enlightened sphere when, aside from the powers of inheritance, education is allotted its true place. The stamp of the miraculous, then, has to give way to an understanding of cause and effect. "Genius is application," Goethe says, and this could be accepted if it be borne in mind that this idea expresses only a half truth, though certainly no untruth. Weininger approaches truth more closely when he says, "Genius is memory," provided he does not mean by this an accumulation of inert informations and dexterities, but rather their vivid and easy interweaving and annexation in our cerebral cortex. But the whole truth is conceived or surmised only if the memory of the race is also taken into consideration—the hereditary memory (*mneme*) whose deciding part is not only conceived by scientists like Hering and Semon (1909,

1921), but also surmised by an author like Jack London in his *White Fang* and *Before Adam*.

Genius can be acquired, but hardly by one individual and within the course of one generation. And genius is hereditary, though probably not without the contributory coöperation of the environment. It is necessary that favorable conditions of heritage and environment meet, in order to make possible the great achievements in which genius manifests itself to our eyes. The necessity of such a combination explains the rarity of recognized genius, and also the fact that many more unrecognized geniuses than we dream of tread the soil of our planet. The rarity of recognized genius manifests itself in an inverse ratio to the furthering with which he endows humanity, and the latter again is in inverse ratio to the assistance which humanity extends to the genius. Small wonder if genius does not bless us more frequently and more lastingly than with short, sporadic flashes of its warmth and radiance!

We would like to change all this, so as to enjoy the blessings of genius more frequently. Materialists and idealists alike should unite in the desire to recognize genius in time and to develop it systematically, instead of leaving it unrecognized and unutilized.

A physician, endeavoring to stem a plague, must first learn how to bring about this scourge of his own volition. There is no other way in which to detect the origin of a disease. Only an understanding of the origin makes prophylaxis and therapy possible. Therefore, let us first analogously consider the reasons which are to be held responsible for the rarity and apparent non-inheritance of genius.

1. Genius is no elementary characteristic, but rather a combination of superior characteristics which only now and then combine in an individual. In accordance with Mendel's Law of Segregation, this complex of characteristics in the progeny is again dissolved into its original elements, provided that the genius begot a progeny, because:

2. Genius, so prolific mentally, is very often sterile in a physical respect. And this is not at all in contrast to the observation that frequently genius is decidedly erotic. For certain reasons (which may be found in my book on *The Prolongation of Human Efficiency*), the erotic urge and its intense application is absolutely independent of the power of propagation. Many poisons, such as alcohol and others, while intensifying this erotic urge, also lessen the propagative power. It seems that the toxins of fatigue work similarly to intoxicating drugs and are passed on from an overworked brain to the germ plasm. The erotic urge may be stimulated, but, at the same time, the propagative result is diminished or deteriorates. For this reason, genius, as a rule, does not beget any progeny, or else only an inferior one.

3. For the inferiority of the progeny of genius, there is another reason besides the two enumerated thus far. Where there is much light, there is bound to be deep shadows and, as the French say, "extremes meet." Genius and insanity, genius and criminality, very often dwell close to each other. The Italian psychiatrist, Lombroso, knew this, but he drew wrong conclusions from it. Lately, Alfred Adler, of Vienna, was more successful in this respect when establishing the theory of organic inferiority. Aside from bril-

liant, captivating characteristics, genius very often displays disagreeable and petty traits of character. Therefore, the already-mentioned Law of Segregation very often proceeds unjustly when parceling out these gifts among the progeny, endowing one child with the most valuable qualities of the progenitor, while another is burdened with inferior characteristics. The latter very often are of a stronger hereditary power and, for this reason, it becomes questionable whether in the progeny that may possibly be born, superior characteristics will predominate.

4. Do not let us forget (and especially in reference to the power of inheritance) that it takes two to beget a child. If the man is highly talented, the woman will very rarely be equally endowed, and *vice versa.* But average usually predominates! The one average parent frequently outweighs the hereditary influence of the more gifted one.

5. Finally, genius, in order to reach its ultimate stage of development, must be given a fair chance. As pointed out before, there is more genius in this world than generally surmised. But of what use is a gift for directing armies to a man standing behind the counter? What benefit is there in the celestial flight of a born poet if he has to tinker in some workshop? There are certain kinds of *general* talents which almost everywhere create satisfactory results, even if put to an inferior use when they are not properly exploited. But other kinds of *specific* talents are much harder hit; on accident alone usually depends their most desirable exploitation. The superstition that genius, under any and all circumstances, asserts itself, even if the individual's health is greatly taxed by deprivations and

hard work, is utterly unworthy of a century which prides itself on its "enlightenment." Carlyle still could venture to say that genius possibly, aye, certainly is endowed with the ability to assert itself, but even the assertion of genius, in reality, is nothing but an example of subtraction: Power of production minus obstacles.

If there is nothing left, genius sinks into oblivion and contemporaries and posterity mourn an irreparable loss. It is the very nature of genius, founded in its unworldliness, its being-so-different-from-anything-else, that makes for its easy eradication by antagonistic conditions. Genius is so much more easily abandoned by its contemporaries and left to outer darkness than is the average. The commonplace belief that anything good will assert itself in the end is utterly wrong when applied to genius.

Looking over the five classes of obstacles as enumerated above—and these five points do not exhaust at all the many adversities that confront genius—it not only becomes clear to us why genius only rarely asserts itself, but we are astonished that genius asserts itself at all. To be sure, there remains the possibility of eliminating these obstacles. Education and eugenics have to coöperate in order to detect genius in time and to systematically develop it.

Education alone, however, would be inadequate. Only lately, Alfred Hock (Prague), methodically elaborating upon Goethe's contention that "genius is application," says that almost *every* human being, if not decidedly inferior, has within himself the makings of great accomplishments. But in spite of being "called, but a few are chosen" because only now and

then an individual, by a caprice of Fate, is placed in a position where he can exhaust his gifts most effectually. Supported by many proofs, Hock explains that only the very earliest application and the most untiring practice will be of deciding value.

It is hardly blasphemous to compare athletic expertness to genius. It is true that we often find ourselves speechless with admiration when facing a physical masterpiece. Here it is a commonplace that a master never drops from heaven, that his marvelous abilities are not inborn. Of course, children born weak or deformed would never develop into athletes; but on the other hand, athletic efficiency can be achieved only by training, begun at an early age.

This is on what Hock bases an "environmental theory of talents" which—because it is an environmental theory and because a prophet is never a prophet in his own country—unfortunately has not been given the place it deserves.

But it is just this point of view which offers the most important assistance in awakening dormant talents and developing them. Certainly, genius to some extent can be acquired by diligence, without which it cannot be exploited. But in the general run of the schools of our day, a kind of diligence is demanded and encouraged which surely does not lead to a very high goal. Boelsche called the one kind of diligence "industry by force"; thus differentiating it from "diligence by talent." What a child likes to do frequently furnishes us with a clew as to the direction in which he is gifted; but this gift very often, proliferating at the expense of other more scholastic studies, is unmercifully suppressed. Classes for gifted pupils and voca-

tional advice, if conducted efficiently, point a way along which we may expect to gradually arrive at a more dependent recognition of talents and a better exploitation of inborn gifts.

This hope, to be sure, is diminished by stubborn and willful misunderstanding. On my arrival in this country (1923), a number of American newspapers insisted that I claimed to have detected a method "to breed genius"—an assertion which hardly seems substantiated by all the obstacles enumerated in the course of this chapter, some of which are almost insurmountable, and up to now have not been surmounted. On the other hand, there were newspapers * which took exception and insisted that my opinion that genius is determined by inheritance is wrong; that only environmental conditions are of deciding value, and for this reason, "the people of the United States are confident that Dr. Kammerer's new environment will make of him even a greater genius than he is."

Incidentally, in this way, my own opinion—or at least an essential part of it—is exploited against its originator himself! And just because I admit that environmental conditions are of great influence, I am also made the target of attacks by the opponents of this theory. Of course, my environmental theory, amending Hock's theory, needs to be supplemented by a theory of the inheritance of genius. Whatever an individual absorbs from environmental conditions is surely not sufficient for attaining that stage of perfection which in great achievements fills us with an awe of almost superhuman accomplishments. Birth is not altogether of no account. Our direct and earlier fore-

*Among others, *The Mirror* of Manchester, N. H., Dec. 4, 1923.

bears should not have stood empty-handed at the cradle if they desired their child to grow up into a "great man."

But there is still the question to be answered: Is genius hereditary or not? The enumeration of the adversities which genius faces in this world may have led to the conclusion: Genius *may* be hereditary . . . and may not. Only under favorable circumstances, and not under unfavorable, genius may be passed on to the progeny. However, this conclusion is wrong: Everything, under almost any circumstances, is inherited! Is the Law of Gravity rendered null and void, simply because an object, speeding towards the center of our planet, is blocked in its course when striking some obstacle in its path? With this obstacle removed, the Law of Gravity once more asserts itself. With the obstacles removed, the Law of Inheritance will once more assert itself!

May the world wake up to the duty of removing these obstacles which confront the visible inheritance of genius! No utopian "Institution for the Ennobling of the Race" (Chapter XLII), nor undignified encroachments upon personal liberty and self-determination is necessary for this end. The only thing necessary is an uplifting environment accessible to everyone and an improved, more individualized education and training. These two coöperatively will take care of eugenic essentials. Both will automatically make the choosing of a mate less difficult, and finally result in an ennobled progeny.

CHAPTER LIII

PRODUCTIVE EUGENICS

WHAT has been explained in Chapter XLVI in reference to an improvement of breeding domesticated animals and raising farm products, also holds true regarding the breeding and raising of human beings with the intention of improving the race. Similar to agriculture, eugenics is also under the spell of the theory of selectionism.

Let us verify this assertion on the basis of legalized eugenic measures, applied in one or more of the United States of America. In his book, *Die Rassenhygiene in den Vereinigten Staaten* ("Race Hygiene in the United States"), Géza von Hoffman classifies these measures as "applicative" and "instructive."

There are three different applicative measures: rules and regulations pertaining to marriage, sterilization of inferior elements, and selection in reference to immigration.

The measures regarding marriage consist of legal requirements, the intention being to exclude such individuals from marrying as suffer from diseases (for example, syphilis) which would deteriorate the race. The possibility that the race suffers from such diseases outside of the bonds of legal matrimony is also taken care of, and if not entirely prevented is at least guarded against to a noticeable degree.

Selection, in reference to immigrants, consists in a medical examination and observation. If immigrants are suffering from diseases which in any way could endanger the people of the United States, they are excluded or deported.

The sterilization of inferior individuals in some of the states (for example, in Michigan) is effected by ligating the spermatic duct. This, at the same time, is one of the previously hinted at measures to amend the prohibition of marriage, because a transgression of these measures is rendered impossible by sterilization.

Very often this sterilization has a curative effect on the individual in question, changing him into a useful member of society, even though he is robbed of his propagative powers. Many criminal urges are rooted in the sex life, or somewhere else within the system of the ductless glands (see Chapter L). If the sex gland is rendered useless, by a ligation of the spermatic duct, then not only is the danger of passing on criminal instincts avoided, but the ductless gland system arrives at a new dynamical equilibrium, with the result that the individual in question recuperates both psychically and physically.

The *instructive* measures consist primarily in the fact that in the majority of the American schools, even in the elementary schools, biology is taught. Within the scope of biology, the basic facts of race-hygiene are expounded. Unfortunately, the so-called "fundamentalists," led by William Jennings Bryan and clergymen of different denominations—it seems unbelievable, but it is the sad truth—have succeeded in excluding evolu-

tion of man from the curriculum of the schools of North Carolina and Kentucky.

Just a glance at the applicative measures suffices to show that, all the way through, they are of nothing but selective nature. These measures endeavor to eliminate the bad, but not to produce the good. We will presently have to investigate how far the way to the production of something positive is cleared by eliminating the negative. But, *per se,* the rules and regulations enumerated so far are purely eliminative and, for this reason, negative. Instructive measures, like lessons in hygiene, would, of course, have to be considered as the beginnings of a positive, *i.e.,* productive eugenics; but for the time being they essentially teach nothing more than the enumerated, prohibitive measures and, for this reason, are just as little productive as those measures themselves.

Let us understand clearly, in the interest of society, what is achieved by selective measures. As an illustration of my point, let us take the population of a city or a country consisting of individuals who are tall, of medium height, and short. Let us further imagine that marriage between individuals who are short or of medium height were either prohibited or effectually prevented, and only giants were permitted to marry. As the approximate size of one's body is hereditarily fixed, the short individuals, the "bantams," would already be eliminated in the next generation. There would still be individuals of medium size, for these would not be entirely eliminated from the heritage and the germ plasm of the contemporaneous generation. But as they would not be allowed to marry, after a few generations only giants would be left.

Is this nation of giants utterly different from the mixed population of which they, at one time, were a part? Did not selection of the giants, in this case, succeed in creating something positive?

Both questions have to be answered in the negative, because the remaining giants are not taller than the tallest of the mixed population originally were. The difference between the time before selection and after is not an intrinsic difference, but rather a difference as to grade only, because *before* all grades of height were represented among the race, with the giants only the tallest and perhaps rarest; but *now* there are only giants. The entire population consists of them and they are now just as numerous as were all the combined different grades previously.

This goes to show that, by selection, a decided change of the population could be brought about, but only in reference to their composition, not regarding their other characteristics. Nothing essentially new would be created. In accordance with Johannsen: From a mixture (*phænotype*), the pure strain (*biotype*) of the very tallest was isolated. We simply bred a generation of giants, but we did not create them.

Let us further suppose that the inhabitants of this city or country were facing the necessity of accomplishing some public work for which the tallest were not even tall enough. The commonwealth would be in need of individuals taller than the tallest now alive or still dormant, with tendencies towards giant-growth, in the germ plasm of the contemporaneous generation. At the very moment when faced with such a task, selection would be revealed as powerless. Selection falls down when required to accomplish something bigger

than that which already exists. Selection is able to monopolize and distribute, but never beyond the primary stage.

To achieve the latter, *i.e.*, the intensification of pre-existing characteristics, we have to resort to positive measures. In the case in question, we would have to try to surpass the existing maximum size, by suitable nourishment, athletics (orthopedic stretching exercises) and systematic feeding of glandular extracts to encourage giant-growth. Only such measures would amount to productive eugenics!

In accordance with opinions laid down in previous chapters of this book, this productive eugenics would presumably not only result in developing a few individuals taller than the tallest were up to now, but would also effect a new hereditary type, as regards size of the body, thus creating a new hereditary tendency towards giant-growth. Under favorable circumstances, this giant-growth could be perpetuated by a repetition of the productive eugenical process.

Here is another way to make the difference between negative and positive, selective and productive eugenics clearer: In all those geological formations which are not of volcanic origin, but were rather effected by floods, resulting in geological strata, fossils are found, remnants of earlier periods of evolution, consisting of bones, shells, ammonites, etc. It is necessary to attack these witnesses of bygone periods with a hammer to get hold of organic relics, to free the latter of the layers of stone which, in the course of time, gradually enveloped them. Not unlike the hammer of the geologist, selection works when, out of a mixed population, the giants and the dwarfs, the heaviest and

the slightest, the strongest and the weakest, the most intelligent and the most stupid, are to be chosen.

No geological hammer will ever accomplish what the chisel and the nimble fingers of a sculptor will create out of a slab of unhewn marble—a statue, a piece of art which was never there before. Analogous to the creating chisel of the sculptor, the powers of environment actually mold a living being and not merely lay bare that which was really there before. Adaptation to environment develops a new body and new determinants, instead of merely developing such tendencies as were already hidden, waiting to be resurrected.

From these metaphors, let us now return to practical eugenics. Let us suppose that we had succeeded in eliminating everything detrimental to the race, such as venereal diseases and criminal instincts. Doubtless a population so changed would have been altered as to structural foundation. It has been freed of inhibitions which formerly tended to form an obstacle to its higher perfection, but it has not, as yet, succeeded in attaining this perfection, despite the elimination of the inferior elements. This race became more perfect only because detrimental elements were disposed of but it did not acquire any new characteristics which its better elements did not possess before.

But perhaps this same population is now able to add new valuable characteristics to the selected, old ones. Freed of the ballast of inferior elements, it is now capable of developing further. This development and adaptation must be clearly differentiated from the process of cleansing and sifting. To fulfill the new tasks, new acquisitions, new investments in reference to tendencies, are necessary which, in turn, will advance

the progress of the race and lift it beyond its previous stage.

I hope it is understood now what is meant by "negative" or selective eugenics on the one hand, and "positive" or productive eugenics on the other. And because I term the weeding out of detrimental characteristics "negative" eugenics, it was stated * that I am criticizing and condemning eugenics, as it stands today. However, this is not the case. I emphatically claim that, by suitable sifting processes, much is to be gained for the race; but I do not think that this should be the end of it. I insist that knowledge and ability even today permit a perfection of the sifting process by creative processes. But first, the prejudice against the inheritance of acquired characteristics prevailing among many of the learned has to be overcome; then the way to productive eugenics, and with that, to a better age for humanity, lies open before our very eyes.

* Among others *The Daily Express,* London, May 1, 1923; sub "Eugenics Superseded."

CHAPTER LIV

ASPECT

THIS understanding, the far-reaching importance of which can hardly be surmised, opens up an entirely new way to improve our own race, to better and benefit mankind as a whole. This is a much more beautiful, a much more dignified path than the one that fanatic race promoters—especially in war-torn Europe—ever and again propose, a method which is based on nothing but the unmerciful struggle for life. Through race-hatred and the elimination of entire races—many of them have incurred the dislike of the one or the other race for no reason whatsoever—we will never succeed in preventing humanity from becoming degenerated. Along this line of march we will never succeed in reaching an ever higher goal, but only by striving to improve ourselves.

If acquired characteristics, if impressions in the life of the individual are hereditary under certain circumstances, we may assume that the deeds and thoughts of man may be passed on in the same way. The responsibility for what we do, and what we do not do, grows bigger than we ever dared to surmise as soon as we become conscious of the fact that our experiences are surely more than just irrelevant for our organic heirs.

This intensified responsibility rests most heavily upon the powers which direct education and govern-

ment. Education, culture, schooling and training no longer exhaust their meaning where we surmised their limits to be, for not only are we benefiting the living individual, but also in our disciples and pupils we improve the bodies and the minds of yet unborn generations. The present of a nation, of a commonwealth, is the more important as its organic future depends on it, as the yet unborn generations have still to be guarded against misrule, against exploitation, against war and the monopolized distribution of the means of a poisonous epicureanism.

And not only teachers and officials, but each individual of every vocation, must understand this new responsibility. Everybody has to work for development, for regeneration; and everybody should lend a hand to this work. The most insignificant action in daily life offers enough occasion for this. We face the necessity of developing new ethics; a new conscience has become necessary—a conscience of the generation and of the race, but never in that antiquated form of a *single* race striving to forge aggressively ahead at the expense of other races.

From this understanding of new responsibilities, new duties also arise for the scholar in all fields of endeavor, because it is the scholar to whom the mental riches of the world are entrusted. The latter is not always aware of this responsible position, as Europe's condition of today shows to such a deplorable extent. We still may observe in the universities of Europe, especially in Germany and France and even in Austria, a most unfortunate outburst of nationalistic movement, a relapse to race-hatred and revenge-propaganda. Developments such as these naturally tend to poison the

whole of spiritual life, to diffuse even science itself with political suppositions. The teachers and professors are mostly to blame for this. The undergraduates are usually misled only by these who cautiously remain in the background, but are nevertheless always prepared to support those who advocate war and barbarism. As yet, what Goethe says is true:

> America, you're better found
> Than we, on our historic ground;
> At least you've got no ruined walls!
> So follow where the moment calls.
> And if your children take to verse,
> May Heaven save them from the curse
> Of knights and robbers, ghosts and ghouls.

Time and again I am asked: "How are we to make our children better human beings, and in that way improve our race?" It is expected of me and my theories that I must be able to point out entirely new revolutionary means and methods and a constructive program. But such is not necessary at all. The old methods of education and child improvement are quite adequate. What really is necessary is the perception which ought to spur us on to distinguish, even more painstakingly than ever before, between the good and the bad, between that which is constructive and that which is destructive. Whatever we perceived up to the present time as good and constructive will remain good and constructive. This holds true for the future also; but we have to resolve to develop the one to a still greater degree and more carefully avoid and destroy the other.

Up to now we were scarcely aware of the fact that

not only "the sins of the fathers are visited upon the children unto the third and the fourth generation," but also that the virtues of the parents may be passed on to subsequent generations. We did not know that individual progression means progression of the race, and that individual retrogression, in time, amounts to racial retrogression. Even though a pupil of whom we expected much remains childless, yet he is sure to influence many others, by the way he lives his life, in accordance with the education with which we endowed him, and among those thus influenced there are sure to be many who will not remain childless. Somewhere the physiological moment arises where tradition is transformed into inheritance. Sooner or later, possibilities will arise when a result transmitted from individual to individual will penetrate into the generation.

The reader is going to ask me now: "Where can we observe results of such generative effects? Must not every child start all over again? Must not every child learn to walk and to talk, to read and to write for himself? Where are there traces of inherited tendencies from those who, before him, had learned to walk and to talk, to write and read?"

One should not be misled. A child, left to itself, will assume an upright carriage of its own volition and, if necessary, this child will even learn to walk unassisted. In Chapter I, we mentioned the inheritance of the teeth. Is not every child born without teeth, and are we not always anxious to make the painful process of teething as easy as possible for it, in spite of the fact that we know that the teeth, already imbedded in the jaw, will break through some day even without our assistance?

And what was told in Chapter I about the childish invention of independent languages also holds good as far as the acquisition of the knowledge of reading and writing is concerned. And yet, "no master has dropped from heaven," as the saying goes; but the disposition to grow into a master is hereditarily transmitted to the child. Certainly, we have to take into consideration the fact that schooling has been compulsory only for a few generations, and has not as yet had a chance to engrave its hereditary traces very deeply into the present generation. But in spite of this period being comparatively limited, we may already claim that children of educated parents usually learn much more quickly than children whose parents were unschooled and intellectually untrained.

The understanding of the heredity of acquired characteristics brings us a message of salvation—a message which, like the old Oriental teachings of Karma and the ethics of Carlyle, believes that no deed can ever be undone. As Carlyle says:

"It is a high, solemn, almost awful thought for every individual man, that his earthly influence, which has had a commencement, will never through all ages, were he the meanest of us, have an end! What is done is done; has already blended itself with the boundless, ever-living, ever-working Universe, and will also work there, for good or for evil, openly or secretly, throughout all time. But the life of every man is as the well-spring of a stream, whose small beginnings are indeed plain to all, but whose ulterior course and destination, as it winds through the expanses of infinite years, only the Omniscient can discern. Will it mingle with neighbouring rivulets, as a tributary; or receive them as

their sovereign? . . . We know not; only in either case, we know, its path is to the great ocean; its waters, were they but a handful, are here, and cannot be annihilated or permanently held back."*

"Beautiful it is to understand and know that a Thought did never yet die; that as thou, the originator thereof, hast gathered it and created it from the whole Past, so thou wilt transmit it to the whole Future. It is thus that the heroic heart, the seeing eye of the first times, still feels and sees in us of the latest; that the Wise Man stands ever encompassed and spiritually embraced, by a cloud of witnesses and brothers; and there is a living, literal Communion of Saints, wide as the World itself, and as the History of the World. Noteworthy also, and serviceable for the progress of this same Individual, wilt thou find his subdivision into Generations. Generations are as the Days of toilsome Mankind: Death and Birth are the vesper and the matin bells, that summon Mankind to sleep, and to rise refreshed for new advancement. What the Father has made, the Son can make and enjoy; but has also work of his own appointed him. Thus all things wax, and roll onwards; Arts, Establishments, Opinions, nothing is completed, but ever completing."†

"It is a great truth, one side of a great truth, that the Man makes the circumstances, and spiritually as well as economically is the artificer of his own fortune. But there is another side of the same truth, that the man's circumstances are the element he is appointed

* *Critical and Miscellaneous Essays,* Vol. 1 (London, Chapman & Hall, 1894), p. 358.

† *Sartor Resartus* (London, Chapman & Hall), p. 167.

to live and work in; that he by necessity takes his complexion, vesture, embodiment, from these, and is in all practical manifestations modified by them almost without limit; so that in another no less genuine sense, it can be said Circumstances make the Man."*

Our life can bring on hereditary degeneration if unhealthy circumstances prevail, if passions—too much on one side and too little on the other—destroy our body and thereby our propagative cells. The active acquisition of certain improving characteristics, however, gives us the power to hereditarily regenerate ourselves. A faculty which we acquire, a dexterity which we train sufficiently, disease and worries which we withstand and conquer, leave traces upon our children and our children's children. Even if ever so diminished; even if apparently extinct for many generations, until a final summing up makes the result known; even if only passed on as a tendency and not at all as a complete achievement; some faint reflection of that which we have been, of that which we achieved, must surely be handed down to those who come after us. A certain extract will reach the spot where man truly is immortal, into that miraculous germ substance out of which an uninterrupted succession of children and children's children are created. The methodical development of man sublime seems a possible and rational task for the future. Call him "man sublime" and not "superman", with a hand clenched to a fist as Nietzsche saw him and desired him to be. But even Nietzsche said at another time: "Not only onward shalt thou propagate thyself, but upward! For that purpose may the garden of marriage help you."

* *Critical and Miscellaneous Essays,* Vol. II, p. 458.

If I permit my scientific conviction to ring out in such a message of cheer, I beg to be permitted to shield myself against the reproach that I am deluded by a sterile optimism. I am not at all merely optimistically inclined. Do not let us forget: The heredity of acquired characteristics is a means, the application of which is left to us. It is left to us to use it for regenerative or degenerative ends. Especially in Europe, one does not gain the conviction that we are on our way to that commonwealth of common sense which will use every means at its disposal to do constructive work, instead of making use of antiquated means of might which lead to nothing but débâcle and destruction.

If I am asked what I expect as the most probable future of our species, my view does not sound so very optimistic at all. History may once more observe the overthrow of a few old established civilizations just as, at one time, rising young nations overthrew the civilization of the Orient, the civilization of Greece and Rome. It may happen that just as at the time of the migration of peoples, the Slavic nations pushing ahead from the East to the West of Europe will overthrow the Teutonic nation; to be, perhaps, in turn overthrown by the peoples of Mongolia. But the wars, which will be thus brought about, will accumulate products of degeneration to such an extent that man will suffer the same fate that other species suffered in the course of the history of our earth; man will vanish, because extinction of the species always follows degeneration of the species.

But there still appears to be one chance: if we would only avail ourselves of the possibilities that offer them-

selves, which are laid bare to our consciousness, to our dignity as human beings, to our scientific insight. I speak only of possibilities, not of utopian certainty, if I voice the opinion that our children and our children's children will much more speedily attain what once we diligently acquired. It will be easier for them to execute what we mastered after hard training; they will survive easily to what we almost succumbed. What we looked for, they will find; where we could make only a beginning, for them it will be happy consummation; and where we battle with victory still uncertain, they, let us hope, will conquer.

REFERENCES

REFERENCES

ABEL, OTHENIO, *Grundzuege der Palaeobiologie der Wirbeltiere*. Stuttgart, 1912.

ABEL, BRAUER, DACQUE, DOFLEIN, GIESENHAGEN, GOLDSCHMIDT, R. HERTWIG, KAMMERER, KLAATSCH, MAAS, SEMON, *Die Abstammungslehre*. 12 gemeinverstaendliche Vortraege. Jena, Gustav Fischer, 1911.

ADLER, ALFRED, *Studie ueber Minderwertigkeit von Organen*. Berlin-Wien, Urban & Schwarzenberg, 1907.

ADLER, LEO, "Metamorphosestudien an Batrachierlarven." *Archiv fuer Entwicklungsmechanik* XXXIX, pp. 21-45, 1914; XL, pp. 1-32, 1914.

ALBINUS, quotation according to R. Semon, 1913.

ALLEN, C. Z., "The Effect of the Age of Sire and Dam on the Quality of Offspring in Dairy Cows." *Journal of Heredity* XIII, Nr. 4, pp. 167-176, 1922.

ALLEN, G. M., "Pattern Development in Mammals and Birds." *American Naturalist* XLVIII, 385, 467, 550, 1914.

ALVERDES, FRIEDRICH, "Die Rolle einer kumulativen Nachwirkung in der Stammesgeschichte." *Zeitschrift fuer induktive Abstammungs- und Vererbungslehre* XXVII, pp. 52-65, 1921 a.

———, "Erblichkeit und Nichterblichkeit." *Naturwissenschaftliche Wochenschrift*, N.F. XX, pp. 377-381, 1921 b.

AMMON, OTTO, *Die Gesellschaftsordnung und ihre natuerlichen Grundlagen*. Jena, 1896.

BABAK, EWALD, "Einige Gedanken ueber die Beziehung der Metamorphose bei den Amphibien zur inneren Sekre-

tion." *Zentralblatt fuer Physiologie* XXVII, p. 536, 1913-14.

BATESON, W., *Problems of Genetics.* New Haven, London, Oxford, Yale University Press, 1913.

———, "Dr. Kammerer's Alytes." *Nature* (London), p. 344, July, 1919; Nr. 2796, p. 738, June 2, 1923; Nr. 2800, p. 878, June 30, 1923; Nr. 2811, p. 391, Sept. 15, 1923, and Nr. 2825, p. 899, December 22, 1923.

BAUR, ERWIN, "Pfropfbastarde, Periklinalchimaeren und Hyperchimaeren." *Berichte der deutschen Botanischen Gesellschaft,* p. 603, 1909, and *Biologisches Zentralblatt* XXX Nr. 15, 1910.

———, *Archiv fuer soziale Hygiene* VIII, p. 122, 1913.

———, "Bemerkungen zu Kammerers Abhandlung: Vererbung erzwungener Farbveraenderungen IV." *Archiv fuer Entwicklungsmechanik* XXXVIII, Nr. 4, pp. 682-683, 1914.

———, *Einfuehrung in die experimentelle Vererbungslehre.* Berlin, Borntraeger, 4. Auflage, 1920. 2. Auflage, 1914, p. 57.)

BECKER, F., "Beobachtungen am Feuersalamander." *Blaetter fuer Aquarien-und Terrarienkunde,* 1914.

BEEBE, C. W., "Geographic Variation in Birds, with Especial Reference to the Effects of Humidity." *Zoologia, Scientific Contributions of the New York Zoological Society,* I, 1, 1907.

BEHRING, *Zeitschrift fuer Hygiene und Infektionskrankheiten* VI, 1889.

BIERENS DE HAAN, J. A., "Die Koerpertemperatur junger Wanderratten (Mus decumanus) und ihre Beeinflussung durch die Temperatur der Aussenwelt." *Archiv fuer Entwicklungsmechanik* L, pp. 1-12, 1922.

——— and PRZIBRAM, HANE, "Erniedrigung der Koerpertemperatur junger Wanderratten (Mus decumanus) durch chemische Mittel und ihr Einfluss auf die Schwanz-

laenge." *Archiv fuer Entwicklungsmechanik* L, pp. 13-40, 1922.

BLARINGHEM, L., "Mutation et Traumatismes." *Bulletin Scientifique de la France et de la Belgique*, Paris, 1907.

———, "Production d'une variété nouvelle d'épinards, Spinacia oleracea var. polygama." *Comptes Rendus de l'Académie des Sciences*, Paris, 147, 1908.

———, "Production par traumatisme d'une forme nouvelle de mais a feuilles crispées." *Comptes Rendus de l'Académie des Sciences*, Paris, 152, 1911.

BODE, "Vererbung erworbener Eigenschaften." *Neue Weltanschauung*, 1912.

BOELSCHE, W., *Stirb und Werde*. Jena, E. Diederichs, 1913.

BOLLEY, H. S., "The Importance of Maintaining a Constant Elimination Factor in Association with a Constant Nutrition Factor in Plant Breeding." *Proceedings American Breeders' Association*, Vol. VIII, pp. 508-514, 1912.

BONNIER, G., "Recherches experimentales sur l'adaptation des plantes au Climat alpin." *Annales sciences naturelles, Botanique*, 7. Série, 1894. See also *Comptes rendus*, vol. 129, 1899.

BORDAGE, E., "A propos de l'heredité des caractères acquis." *Bulletin scientifique de la France et de la Belgique*, 7. Série, tome 54, Paris, 1910.

BORN, G., "Verwachsungsversuche mit Amphibienlarven." *Archiv für Entwicklungsmechanik* IV, 1897.

BOULENGER, E. G., "Experiments on Color Changes of the Spotted Salamander (Salamandra maculosa) Conducted in the Society's Garden." *Proceedings of the Zoological Society of London*, pp. 99-102, 1921.

BRAUN, H., "Ueber die Wirkung der Unterernaehrung auf Bakterien." *Zeitschrift fuer allgemeine Physiologie* XIX, pp. 1-8, 1921.

———, and TEICHMANN, E., "Ueber Trypanosomen-Immuni-

sierung." *Berliner klinische Wochenschrift* XLVIII, p. 1562, 1911; *Deutsche medizinische Wochenschrift* XXXVIII, p. 107, 1912; *Beihefte zum Archiv fuer Schiffs- und Tropenhygiene* XVI, pp. 427-433; *Zentralblatt fuer Bakteriologie* 1. Abt., LIV, pp. 11-16, Referat, Beilage, 1912.

BRAUS, H., "Vordere Extremitaet und Operculum bei Bombinator-Larven." *Morphologisches Jahrbuch* XXXV, 1906, and *Experimentelle Beitraege zur Morphologie* I, 1906.

BRECHER, LEONORE, "Die Puppenfaerbung des Kohlweisslings Pieris brassicae L." *Archiv fuer Entwicklungsmechanik*, 1917–1922.

BRESCA, GIOVANNI, "Experimentelle Untersuchungen ueber die sekundaeren Sexualcharaktere bei Tritonen." *Archiv fuer Entwicklungsmechanik* XXIX, Nr. 3-4, 1910.

BROWN-SÉQUARD, "On the Hereditary Transmission of Effects of Certain Injuries to the Nervous System." *Lancet*, p. 7, 1875.

———, "Heredité d'une affection due a une cause accidentelle, Faits et argument contre l'explication et les critiques de Weismann." *Archives de Physiologie normale et pathologique*, 1892. For a complete list of the papers of Brown-Séquard, treating this subject, see Maciesza and Wrzosek 1911 a. Extensive Report in Charles Darwin, "Variation of Animals and Plants under Domestication."

BRUEL, "Artumbildungs- und Variabilitaetsstudien am Nervensystem von Firoloida kowalevskyi Vayss. (desmaresti)." *Zoologische Jahrbuecher, Abteilung fuer allgemeine Zoologie und Physiologie* XXXVIII, pp. 517-562, 1921.

BUDER, JOHANNES, "Studien an Laburnum Adami." *Berichte der Deutschen Botanischen Gesellschaft* XXVIII, pp. 116-118, 1910, etc.

BUECHNER, LUDWIG, "Die Macht der Vererbung und ihr Einfluss auf den moralischen und geistigen Fortschritt der Menschheit." *Darwinistiche Schriften*, Nr. 12, Leipzig, Ernest Guenther, 1882.

BURBANK, LUTHER, *How Plants Are Trained to Work for Man.* Preface by David Starr Jordan. New York, P. F. Collier & Son, 1921.

BUXBAUM, *Vortrag auf der Versammlung deutscher Naturforscher und Aerzte zu Nauheim*, 1920.

CASTLE, W. E., and PHILLIPS, JOHN C., "A Successful Ovarian Transplantation in the Guinea Pig, and Its Bearing on Problems of Genetics." *Science*, N.S. XXX, pp. 312-313, 1909.

———, "On Germinal Transplantation in Vertebrates." Carnegie Institute Publication, Washington, 1911.

CHARRIN, A., and DELAMARE, G., "Procedé capable de s'opposer a la transmission aux rejetons des tares viscerales maternelles." *Comptes Rendus de la Société Biologique Paris* LVIII, 1. sém., pp. 357-359, 1906.

CHAUVIN, M. v., "Ueber die Verwandlungsfaehigkeit des mexikanischen Axolotl." *Zeitschrift fuer wissenschaftliche Zoologie* XLI, 1885.

CIESLAR, A., "Ueber die Erblichkeit des Zuwachsvermoegens bei den Waldbaeumen." *Zentralblatt fuer das gesamte Forstwesen*, 1895 and 1907.

CONGDON, E. D., "The Surrounding of the Germ Plasm III. The Internal Temperature of Warm-Blooded Animals in Artificial Climates." *Archiv fuer Entwicklungsmechanik* XXXIII, 1912.

CONKLIN, EDWIN GRANT, "Heredity and Environment in the Development of Men," 3d Edition. *Princeton University Press*, 1915.

———, *Direction of Human Evolution.* New York, Charles Scribner's Sons, 1921.

Cope, E. D., *The Origin of the Fittest.* New York, 1887.
———, *The Primary Factors of Organic Evolution.* Chicago, 1896.
Crampton, H. E., Jr., "Coalescence Experiments Upon Lepidoptera." Woods Hole Biological Lectures for 1896-1897, Boston.
———, "An Experimental Study Upon Lepidoptera." *Archiv für Entwicklungsmechanik* IX, 1899.
Cuénot, L., *L'influence du milieu sur les animaux.* Paris, G. Massen, 1894.
———, *La génèse des espèces animales.* Paris, F. Alcan, 1911. See also *Biologica* IV, Nr. 42, 1914, and *Bulletin de la Société biologique de France* XXXIX, p. 83, 1914.
Cunningham, J. F., "An Experiment Concerning the Absence of Color from the Lower Sides of Flatfishes." *Zoologischer Anzeiger* XIV, 1891. See also *Natural Science* I, 1892; II, 1895.
———, "Observations and Experiments on Japanese Long-tailed Fowls." *Proceedings of the Zoological Society of London* I, pp. 227-250, 1903.
———, "The Heredity of Secondary Sexual Characters in Relation to Hormones. A Theory of the Heredity of Somatogenetic Characters." *Archiv fuer Entwicklungsmechanik* XXVI, 1908.
———, "Dr. Kammerer's Lecture to the Linnean Society," etc. *Nature* (London), Vol. CXI, No. 2795, p. 702, May 26, 1923; Vol. CXII, No. 2804, p. 133, July 28, 1923; Vol. CXII, No. 2824, p. 864, December 15, 1923.

Darwin, Charles, *The Origin of Species by Means of Natural Selection.* London, 1859.
———, *Variation of Plants and Animals Under Domestication.* London, 1875.
———, *Naturalist's Voyage Round the World.* New edition, New York, D. Appleton & Co., 1883.

DARWIN, L., "Environment as a Factor in Evolution." *Eugenics Revue* X, pp. 63-70, London, 1918.

DAVENPORT, C. B., "Inheritance in Poultry." Carnegie Institution Publication, Nr. 53, pp. 43-48, Washington, 1906.

———, "The Transplantation of Ovaries in Chickens." *Journal of Morphology* XXII, No. 1, March 20, 1911.

DEMBOWSKI, JAN, "Das Kontinuitaetsprinzip und seine Bedeutung in der Biologie." *Vortraege und Aufsaetze ueber Entwicklungsmechanik*, Nr. 21, Berlin, J. Springer, 1919.

DEMOLL, R., "Die Vererbbarkeit somatischer Erwerbungen." *Archiv fuer Entwicklungsmechanik* XLVI, pp. 4-11, 1920; XLVII, pp. 443-452, 1921.

DETLEFSEN, J. A., "Are the Effects of Long-Continued Rotation in Rats Inherited?" *Proceedings of the American Philosophical Society* LXII, Nr. 5, 1923.

DETMER, "Lehrbuch der Pflanzenphysiologie," p. 272, and "Zum Problem der Vererbung," *Pfluegers Archiv fuer die gesamte Physiologie* XLI, 1887.

DETTO, C., *Die Theorie der direkten Anpassung.* Jena, G. Fischer, 1904.

DRIESCH, HANS, "Studien ueber das Regulationsvermoegen der Organismen VI. Die Restitutionen der Clavellina lepadiformis." *Archiv fuer Entwicklungsmechanik* XIV, 247-287, 1902.

DUERKEN, B., "Ueber die Wirkung farbigen Lichtes auf die Puppen des Kohlweisslings und das Verhalten der Nachkommen." *Archiv fuer mikroskopische Anatomie und Entwicklungsmechanik,* vol. 99, Nr. 2-4, p. 222, 1923.

DUPUY, E., *Gazette médicale de Paris,* No. 33, 1875, and *Bulletin scientifique de la France et de la Belgique* III, 1890

EHRLICH, PAUL, "Experimentelle Untersuchungen ueber Immunitaet." *Deutsche medizinische Wochenschrift* XVII, pp. 976, 1218, 1891.

———, "Ueber Immunitaet durch Verebung und Saeugung." *Zeitschrift fuer Hygiene und Infektionskrankheiten* XII, p. 183-205, 1892.

ELLIS, HAVELOCK, *A Study of British Genius.* London, 1904.

EWART, J. COSSAR, "Experimental Contributions to the Theory of Heredity. Reversion and Telegony." *Transactions Highland and Agricultural Society of Scotland,* p. 54, 1901.

EXNER, SIGMUND, "Bemerkungen zur Frage nach der Vererbung erworbener psychischer Eigenschaften." *Bericht über den IV Kongress für experimentelle Psychologie in Innsbruck*, pp. 203-210, 1910.

FEJERVARY, G. J. DE, "Sur deux cas interessants d'Adaptation produits par le terrain sur la couleure des animaux." *Bulletin de la Société Vaudoise des Sciences Naturelles Tome* XLVIII, pp. 381-391, 1912.

FERRONIÈRE, *Etudes biologiques sur la faune supralitorale de la Loire Inférieure.* Paris, 1901.

FINKLER, WALTER, "Kopftransplantation an Inskten III. Einfluss des replantierten Kopfes auf das Farbkleid anderer Koerperteile." *Archiv fuer mikroskopische Anatomie und Entwicklungsmechanik*, vol. 99, Nr. 1, pp. 126-133, 1923.

FISCHER, E., "Experimentelle Untersuchungen ueber die Vererbung erworbener Eigenschaften." *Allegemeine Zeitschrift fuer Entomologie* VI, VII, 1901, 1902.

FOREL, A., *Hygiene der Nerven und des Geistes.* Stuttgart, E. H. Moritz, 4th Edition, 1911.

FOX, MUNRO, "Dr. Kammerer's Ciona-Experiments." *Nature*

(London), Vol. CXII, pp. 653-654, November 3, 1923.

FRAENKEL, MANFRED, "Roentgenstrahlenversuche an tierischen Ovarien zum Nachweis der Vererbung erworbener Eigenschaften." *Archiv fuer mikroskopische Anatomie* LXXX, Abt. II, pp. 61-77, 1912.

———, "Die Wirkung der Roentgenstrahlen im Hinblick auf Vererbung und Verjuengung." *Archiv fuer Frauenkunde und Eugenik* VII, 1921.

FRISCH, KARL VON, "Ueber den Einfluss der Bodenfarbe auf die Fleckenzeichnung des Feuersalamanders." *Biologisches Zentralblatt* XL, p. 390, 1920.

GADOW, HANS, quotation according to M. Perkins, *Nature* (London), Vol. CXII, Nr. 2807, August 18, 1923.

GAGE, S. H., "Sudan III Deposited in the Egg and Transmitted to the Chick." *Science*, N.S. XXVIII, p. 494, 1908.

GAISCH, ALOIS, "Ein weiterer Beitrag zur kuenstlichen Schwarzfaerbung des gefleckten Salamanders (Salamandra maculosa Laur.)." *Verhandlungen der Zoologisch-botanischen Gesellschaft Wien* LXII, pp. 54-57, 1912.

GALTON, FRANCIS, "Inquiries Into Human Faculty and Its Development." London, 1883.

GIARD, A., "Le Principe de Lamarck et l'Herédité des Modifications somatiques." *Revue scientifique et Controverses transformistes*, Paris, Masson & Co., 1898.

GLEY, and CHARRIN, A., "Influences heréditaires expérimentales." *Archiv de Physiologie*, 1893, 1894.

GODLEWSKI, EMIL, *Das Vererbungsproblem im Lichte der Entwicklungsmechanik.* Leipzig, W. Engelmann, 1909.

———, "Physiologie der Zeugung," in Winterstein's *Handbuch der vergleichenden Physiologie.* Jena, G. Fischer, 1914.

GOLDSCHEID, RUDOLF, *Hoeherentwicklung und Menschenoekonomie.* Leipzig, A. Kroener, 1911.

GOLDSCHMIDT, RICHARD, *Einfuehrung in die Vererbungswissenschaft.* Leipzig, W. Engelmann, 3d Edition, 1918.

———, "Die quantitative Grundlage von Vererbung und Artbildung." *Vortraege und Aufsaetze ueber Entwicklungsmechanik* XXIV; Berlin, J. Springer, 1920.

GRAFE, VIKTOR, and LINSBAUER, KARL, "Ueber die wechselseitige Beeinflussung von Nicotiana tabacum und N. affinis bei der Pfropfung." *Berichte der Deutschen Botanischen Gesellschaft* XXIV, 366, 1906.

GRIFFITH, C. R., *Science*, N. S. LVI, p. 676.

GROSS, J., "Ueber einige Beziehungen zwischen Vererbung und Variation." *Biologisches Zentralblatt*, 1906.

GUDERNATSCH, J. F., "Feeding Experiments on Tadpoles." *Archiv fuer Entwicklungsmechanik* XXXV, p. 475, 1912, and *American Journal of Anatomy* XV, p. 431, 1914.

GUTHRIE, C. C., "Further Results of Transplantation of Ovaries in Chickens." *Journal of Experimental Zoology* V, 1908. See also *Science*, 1909, 1911.

GUYER, M. F., and SMITH, E. A., "Studies on Cytolysins." I. "Some Prenatal Effects of Lens Antibodies." *Journal of Experimental Zoology* XXVI, pp. 65-82, 1918.

———, "Studies on Cytolysins." II. "Transmission of Induced Eye Defects." *Journal of Experimental Zoology* XXXI, pp. 171-224, 1920.

HAECKEL, ERNEST, *Alte und neue Naturgeschichte.* Jena, G. Fischer.

HAECKEL, THOMSON, WEISMANN and others, *Evolution in Modern Thought.* New York, Boni & Liveright.

HAMMERSCHLAG, V., "Zuchtversuche mit japanischen Tanzmaeusen und europaeischen Laufmaeusen." *Archiv fuer Entwicklungsmechanik* XXXIII, 1912.

HANKO, B., "Ueber den Einfluss einiger Loesungen auf die Haeutung, Regeneration und das Wachstum von Asellus aquaticus." *Archiv fuer Entwicklungsmechanik* XXXIV, p. 477, 1912.

HANSEMANN, D., VON, "Ueber Makrobiotik." *Berliner klinische Wochenschrift*, Nr. 5, 1910.

HARMS, W., "Ueberpflanzung von Ovarien in eine fremde Art. I. Versuche an Lumbriciden." *Archiv fuer Entwicklungsmechanik* XXXIV, pp. 90-131, 1912.

HARRISON, R. G., "Embryonic Transplantation and the Development of the Nervous System." *Harvey Lectures*, 1907–1908, pp. 199-222, Philadelphia and London, 1909. See also *Anatomical Record* II, pp. 385-410, 1908–1909; *Journal of Experimental Zoology* IX, 1910, and XVII, 1914.

HARTMANN, M., "Untersuchungen ueber die Morphologie und Physiologie des Formwechsels (Entwicklung, Fortpflanzung, Befruchtung und Vererbung) der Phytomonadinen (Volvocales)." *Sitzungsberichte der preussischen Akademie der Wissenschaften* LII, pp. 760-776, 1917.

HATSCHEK, B., *Hypothese der organischen Vererbung*. Leipzig, W. Engelmann, 1905.

HAYKRAFT, J. B., *Natuerliche Auslese und Rassenverbesserung*. 3d Edition, Leipzig, 1895.

HEAPE, WALTER, "Further Note on the Transplantation and Growth of the Mammalian Ova Within an Uterine Foster Mother." *Proceedings of the Royal Society of London* LXVII, p. 178, 1898.

HEIDER, KARL, *Vererbung und Chromosomen*. Jena, G. Fischer, 1906.

HERBST, KURT, *Beitraege zur Entwicklungsphysiologie der Faerbung und Zeichnung der Tiere. I. Der Einfluss gelber, weisser und schwarzer Umgebung auf die Zeichnung von Salamandra maculosa.* Abhandlungen

der Heidelberger Akademie der Wissenschaften, mathematisch-naturwissenschaftliche Klasse, 7. Abhandlung, 1919.

HERING, EWALD, *Ueber das Gedaechtnis als eine allgemeine Funktion der organisierten Materie.* 2d Edition, Wien, C. Gerolds Sohn, 1876.

HERTWIG, OSKAR, *Allgemeine Biologie.* 3d Edition, pp. 644-659, Jena, 1909.

———, *Zur Abwehr des ethischen, des sozialen und des politischen Darwinismus.* Jena, G. Fischer, 1918.

HERTZ, FRIEDERICH, *Rasse und Kultur.* Leipzig, A. Kroener, 1915.

HERWERDEN, M. A., VAN, "Der Einfluss der Nebennierenrinde des Rindes auf Gesundheit und Wachstum verschiedener Organismen." *Biologisches Zentralblatt* XLII, pp. 109-112, 1922.

HESSE-DOFLEIN, *Tierbau und Tierleben.* Leipzig-Berlin, Verlag B. G. Teubner, II. Band, 1914.

HIMMER, A., "Untersuchungen ueber den physiologischen und morphologischen Farbwechsel bei Amphibien." *Archiv fuer mikroskopische Anatomie und Entwicklungsmechanik* C, pp. 110-163, 1923.

HIRSCHLER, J., "Ueber die theoretische Fassung des Problems der Vererbung erworbener Eigenschaften." *Archiv fuer mikroskopische Anatomie Bonn* XC, 2. Abt., pp. 243-274, 1917.

HIRTH, GEORG, *Entropie der Keimsysteme und erbliche Entlastung.* Muenchen, G. Hirth's, Verlag, 1900.

HOCK, ALFRED, *Die methodische Entwicklung der Talente und des Genies.* Leipzig, Akademische Verlagsgesellschaft, 1920.

HOFFMANN, GÉZA V., *Die Rassenhygiene in den Vereinigten Staaten von Nordamerika.* Muenchen, Verlag Lehmann, 1913.

HOFFMANN, H., "Beobachtungen ueber thermische Vegeta-

tionskonstanten." *Meteorologische Zeitschrift*, pp. 546-547, 1886.

———, "Kulturversuche ueber Variation." *Botanische Zeitung*, 1887; *Botanisches Zentralblatt* XXXI, p. 37, 1887.

HOGE, MILDRED A., "The Influence of Temperature on the Development of a Mendelian Character." *Journal of Experimental Zoology* XVIII, pp. 241-297, 1915.

HOLMES, S. J., *Studies in Evolution and Eugenics.* Harcourt, Brace & Co., 1924.

HOSKINS, R. G., "Gland Research Aims to Prolong Active Life." New York *Times,* Sunday, January 20, 1924.

HOWLETT, F. M., "Notes on Head- and Body Lice." *Parasitology* X, 186, 1917.

JACKMANN, OTTO, "Ueber die Vorstellbarkeit der direkt bewirkten Anpassungen und der Vererbung erworbener Eigenschaften durch das Prinzip der virtuellen Verschiebungen." *Vortraege und Aufsaetze ueber Entwicklungsmechanik*, Nr. 28, Berlin, J. Springer, 1922.

JANDA, VIKTOR, "Die Regeneration der Geschlechtsorgane bei Criodrilus lacuum.". *Archiv fuer Entwicklungsmechanik* XXXIII, pp. 345-348; XXXIV, pp. 557-587, 1912.

JENNINGS, H. S., "Heredity, Variation and Evolution in Protozoa." *Journal of Experimental Zoology* V, pp. 557-632, 1908, and *Proceedings American Philosophical Society* XLVII, Nr. 190, 1908; *Eugenics, Genetics and the Family* I, 1923.

———, *Life and Death, Heredity and Evolution in Unicellular Organisms.* Boston, Richard G. Badger, 1920.

———, "An Illuminating Method of Handling Data." *Science,* LIX, Nr. 1515, p. 39, January 11, 1924.

JENSEN, C. O., "Partiel Metamorfose hos Amblystoma mexi-

canum." *Vidensk. Meddel. Kobenhavn* LXXII, pp. 173-180, 1921.

JOEST, E., "Transplantationsversuche an Lumbriciden." *Archiv fuer Entwicklungsmechanik der Organismen* V, 1897.

JOHANNSEN, W., *Elemente der exakten Erblichkeitslehre.* 2d German Edition, Jena, G. Fischer, 1913.

JOLLOS, V., "Experimentelle Protistenstudien I: Untersuchungen ueber Variabilitaet und Vererbung bei Infusorien." *Archiv fuer Protistenkunde* XLIII, pp. 1-122, 1921.

KABRHEL, G., "Ueber den Einfluss des Alkohols auf das Keimplasma." *Archiv fuer Hygiene* LXXI, Nr. 1, 1909.

KAMMERER, PAUL, "Tanzende Waldmaus und radschlagende Hausmaus." *Der Zoologische Beobachter* (*Zoologische Garten*) LIV, Nr. 12, 1900.

———, "Vererbung der erworbenen Eigenschaft habituellen Spaetgebaerens bei Salamandra maculosa." *Zentralblatt fuer Physiologie* XXI, Nr. 4, 1907 a.

———, "Erzwungene Fortpflanzungsveraenderungen und deren Vererbung." *Zentralblatt fuer Physiologie* XXI, Nr. 8, 1907 b.

———, "Vererbung erzwungener Fortpflanzungsanpassungen," I., II. "Die Nachkommen der spaetgeborenen Salamandra maculosa und der fruehgeborenen Salamandra atra." *Archiv fuer Entwicklungsmechanik* XXV, Nr. 1-2, 1907 c.

———, "Vererbung erzwungener Fortpflanzungsanpassungen," III. "Die Nachkommen der nicht brutpflegenden Alytes obstetricans." *Archiv fuer Entwicklungsmechanik* XXVIII, Nr. 4, 1909.

———, "Die Wirkung aeusserer Lebensbedingungen auf die organische Variation im Lichte der experimentellen Morphologie." *Archiv fuer Entwicklungsmechanik* XXX (Festband fuer Wilhelm Roux), 1910 a.

———, "Vererbung erzwungener Farbveraenderungen," I., II. "Induktion von weiblichem Dimorphismus bei Lacerta muralis, von maennlichem Dimorphismus bei Lacerta fiumana." *Archiv fuer Entwicklungsmechanik* XXIX, Nr. 3-4, 1910 b.

———, "Vererbung erzwungener Farb- und Fortpflanzungsveraenderungen bei Amphibien." *Verhandlungen der gesellschaft deutscher Naturforscher und Aerzte Salzburg* II, 1, pp. 173-176, 1910 c.

———, "Vererbung erzwungener Farb- und Fortpflanzungsveraenderungen bei Reptilien." *III. Internationaler Physiologenkongress. Umschau* XV, Nr. 7, 1910 d.

———, "Beweise fuer die Vererbung erworbener Eigenschaften durch planmaessige Zuechtung." *12. Flugschrift der Deutschen Gesellschaft fuer Zuechtungskunde*, Berlin, 1910 e.

———, "Neuere eigene Zucht- und Transplantationsversuche ueber Vererbung somatogener Eigenschaften." *Sitzungsberichte der Gesellschaft Naturforschender Freunde,* Berlin, Nr. 10, 1910 f.

———, "Das Beibehalten jugendlich unreifer Formzustaende (Neotenie und Progenese)." *Ergebnisse der wissenschaftlichen Medizin*, pp. 406-434, 1910 g.

———, "Zucht- und Transplantationsversuche ueber Vererbung koeperlich erworbener Eigenschafter bei Tieren und Pflanzen." *Verhandlungen der Zoologisch-botanischen Gesellschaft,* Wien, LXI, pp. 134-139, 1911 a.

———, "Mendelsche Regeln und Vererbung erworbener Eigenschaften." *Verhandlungen des Naturforschenden Vereines Bruenn* XLIX (Festband fuer Mendel), 1911 b.

———, "Direkt induzierte Farbanpassungen und deren Vererbung." *Zeitschrift fuer induktive Abstammungs- und Vererbungslehre* IV, Nr. 3-4, 1911 c, and *Verhandlungen des* VIII. Internationalen Zoologenkongresses Graz, pp. 263-271, 1912 a.

———, "Experimente ueber Fortpflanzung, Farbe, Augen und Koerperreduktion bei Proteus anguineus Laur. (Zugleich: Vererbung erzwungener Farbveraenderungen III.)." *Archiv fuer Entwicklungsmechanik* XXXIII, Nr. 3-4, 1912 b.

———, "Koerperkultur und Rasse." *Das oesterreichische Sanitaetswesen*, Nr. 22, 1912 c.

———, *Erwerbung und Vererbung des musikalischen Talentes.* Leipzig, Verlag Theod. Thomas, 1912 d.

———, "Monistische und dualistische Vererbungslehre." *Das monistiche Jahrhundert*, Nr. 12, 1912 e.

———, "Adaptation and Inheritance in the Light of Modern Experimental Investigation." *Annual Report of the Smithsonian Institution*, 1913 a.

———, "Erbliche Entlastung und gegenseitige Hilfe." Studentenalmanach "Vom Studium und von Studenten." Berlin, Verlag Bruno Cassirer, 1913 b.

———, "Vererbung erzwungener Farbveraenderungen. IV. Das Farbkleid des Feuersalamanders (Salamandra maculosa Laur.) in seiner Abhaengigkeit von der Unwelt." *Archiv fuer Entwicklungsmechanik* XXXVI, Nr. 1-2, 1913 c.

———, "Ueberblick der modernen Variations- und Vererbungslehre." *Monatshefte fuer den Naturwissenschaftlichen Unterricht* VI, Nr. 1, 1913 d.

———, "Koerperplasma und Keimplasma." *Das monistische Jahrhundert*, 1913 e.

———, "Variabilitaet, Variation der Tiere und Pflanzen." *Handwoerterbuch der Naturwissenschaften* X, pp. 181-209, 1913 f.

———, "Nachweis normaler Funktion beim herangewachsenen Lichtauge des Proteus." *Pfluegers Archiv fuer die gesamte Physiologie* CLIII, pp. 430-440, 1913 g.

———, "Die Bedeutung der Vererbung erworbener Eigenschaften fuer Erziehung und Unterricht." *Flug-*

schriften der Sozialpaedagogischen Gesellschaft, Wien, 1914 a.

———, "Bemerkungen zum Laichgeschaeft und der Brutpflege bei der Geburtshelferkroete." *Blaetter fuer Aquarien- und Terrarienkunde* XXV, Nr. 15, 1914 b.

———, "Aufklaerung zu vorstehenden Bemerkungen des Herrn Prof. Baur." *Archiv fuer Entwicklungsmechanik* XXXVIII, Nr. 4, 1914 c.

———, "Erbliche Entlastung." *Wochenschrift Urania,* Wien, IX, Nr. 38, 1916.

———, "Einzeltod, Voelkertod, biologische Unsterblichkeit." Wien und Leipzig, *Anzengruber-Verlag*, Brueder Suschitzky, 1918 a.

———, "Naturforscherreisen zu den Felseneilanden Dalmatiens." *Urania-Buecherei,* III, 2d Edition, Vienna, Waldheim-Eberle, 1918 b.

———, "Vererbung erzwungener Formveraenderungen I: Die Brunftschwiele des Alytes-Maennchens aus Wassereiern (zugleich: Vererbung erzwungener Fortpflanzungsanpassungen V)." *Archiv fuer Entwicklungsmechanik* XLV, Nr. 1-2, 1919.

———, "Allegemeine Biologie." Stuttgart, Berlin, Leipzig, *Deutsche Verlagsanstalt*, 1. Auflage, 1915; 2. Auflage, 1920 a.

———, "Lebensbeherrschung. Grundsteinlegung zur organischen Technik." *Monistische Bibliothek, kleine Flugschriften*, Nr. 13. Hamburger Verlag, 1. Auflage, 1919; 2. Auflage, 1920 b.

———, "Das biologische Zeitalter. Fortschitte der organischen Technik." *Monistische Bibliothek, kleine Flugschriften*, Nr. 33. Hamburg Verlag, 1920 c.

———, "Dunkeltiere im Licht und Lichttiere im Dunkel." *Die Naturwissenschaften* VIII, Nr. 2, 1920 d.

———, *Sind wir Sklaven der Vergangenheit oder Werkmeister der Zukunft?* Wien-Leipzig, Anzengruber-Verlag

Brueder Suschitzky, 1, Auflage, 1913; 2, Auflage, 1921.

———, "Die Zeichnung von Salamandra maculosa im durchfallenden farbigen Licht." *Archiv fuer Entwicklungsmechanik* L, Nr. 1-2, 1922.

———, "Breeding Experiments on the Inheritance of Acquired Characters." *Nature* (London), Nr. 2793, pp. 637-640, May, 1923. See also Nr. 2807, pp. 237-238, August, 1923 a.

———, *The Prolongation of Human Efficiency.* A book on the Steinach Method of Rejuvenation. New York, Boni & Liveright, 1923 b.

Kapterew, P., "Ueber den Einfluss der Dunkelheit auf das Daphnienauge." *Biologisches Zentralblatt* XXX, Nr. 7, 1910, and XXXII, 1912.

Kaufman, Laura, "On the Metamorphosis of Amblystoma Mexicanum Cope Fed on Thyroidine." *Bull. de l'Acad. Sciences Cracovie, Cl. math. et nat.*, Ser. B, pp. 54-56, 1917.

Kellogg, T. H., "Eugenics and Euthenics." *Good Health Magazine,* Battle Creek, Mich., August, 1921.

Kendrick, John F., "The Engineer and the Mystagogues." *Western Medical Times*, July, 1923.

Klebahn, H., *Die wirtswechselnden Rostpilze.* Berlin, 1904.

Klebs, G., "Ueber kuenstliche Metamorphosen." *Abhandlungen der Naturforschenden Gesellschaft* Halle, a. S., XXV, 1906.

———, "Studien ueber Variationen." *Archiv fuer Entwicklungsmechanik* XXIV, 1907.

———, "Ueber die Nachkommen kuenstlich veraenderter Blueten von Sempervivum." *Sitzungsberichte der Heidelberger Akademie der Wissenschaften, mathematisch-naturwissenschaftliche Klasse*, 5, Abhandlung, 1909.

Kohlbrugge, J. H. F., "Die Verbreitung der Spermatozoiden im weiblichen Koerper und im befruchteten Ei."

Archiv fuer Entwicklungsmechanik XXXV, pp. 165-188, 1912.

———, "De erfelijkheid van verkregen eigenschappen." *Genetica, s'Gravenhage*, deel 1, pp. 347-386, 1919.

KOPPANYI, THEODOR, "Theoretische Erwaegungen ueber die Entstehung der Alterserscheinungen und des Todes." *Naturwissenschaftliche Wochenschrift*, N.F. XX, pp. 726-728, 1921.

———, "Die Replantation von Augen." *Archiv fuer mikroskopische Anatomie und Entwicklungsmechanik*, vol. 99, Nr. 1, 1923.

KORNFELD, WERNER, "Abhaengigkeit der metamorphotischen Kiemenrueckbildung vom Gesamtorganismus der Salamandra maculosa." *Archiv fuer Entwicklungsmechanik* XL, pp. 369-415, 1914.

KREYENBERG, "Briefe aus China." *Blaetter fuer Aquarien- und Terrarienkunde*, p. 261, 1909; p. 265, 1911.

KRONACHER, CARL, *Grundzuege der Zuechtungsbiologie.* Berlin, Paul Parey, 1912.

KROPOTKIN, PETER, *Mutual Aid a Factor of Evolution.* 1904. Revised and cheaper edition. London, William Heinemann, 1910.

———, "The Direct Action of Environment in Plants." *The Nineteenth Century,* July, 1910; November, December, 1910, and March, 1912.

KUEKENTHAL, W., "Vergleichend-anatomische und entwicklungsgeschichtliche Untersuchungen an Sirenen." *Semon's Zoologische Forschungsreisen in Australien* IV, 1, Jena, 1897.

LAMARCK, J. J., *Histoire Naturelle des animaux sans vertèbres* (Introduction), 1805, 2d Edition 1835.

LANG, ARNOLD, "Ueber Vererbungsversuche." *Verhandlungen der Deutschen Zoologischen Gesellschaft* 1909.

———, *Die experimentelle Vererbungslehre in der Zoologie seit 1900.* Jena, G. Fischer, 1914.

Laufberger, V., "O vzbuzeni metamorfosy axolotlu krmenim zlazov stitnou." *Biologicke Listy* II, 1913.

Leche, W., "Ein Fall von Vererbung erworbener Eigenschaften." *Biologisches Zentralblatt* XXII, 1902.

Lenz, Fritz, "Die Auslesebedeutung der Saenglingsfuersorge." *Berliner klinische Wochenschrift* LVI, pp. 1139-1141, 1919.

———, "Oscar Hertwig's Angriff gegen den 'Darwinismus' und die Rassenhygiene." *Archiv fuer Rassen- und Gesellschaftsbiologie* XIII, pp. 194-203, 1918-20.

Lesage, P., "Recherches éxperimentales sur les modifications des feuilles chez les plantes maritimes." *Revue générale de Botanique* II, 1890.

Leschke, quotation according to Hoskins.

Linden, Marie von, "Der Einfluss des Stoffwechsels der Schmetterlingspuppe auf die Fluegelfaerbung und Zeichnung des Falters." *Archiv fuer Rassen- und Gesellschaftsbiologie* I, 1904.

Loeb, Jacques, "Aphorismen zur Vererbungslehre." *Das monistische Jahrhundert* I, 1, pp. 6-12, 1912.

———, "The Blindness of the Cave Faune and the Artificial Production of Blind Fish Embryos by Heterogeneous Hybridization and by Low Temperatures." *Biological Bulletin* XXIX, Nr. 1, pp. 50-67, 1915.

———, *The Organism as a Whole from a Physicochemical Viewpoint.* New York, London, G. P. Putnam's Sons, 1916.

London, Jack, "White Fang," pp. 68, 85, 114, 145, 177, 199. New York., The Review of Reviews Company, 1906.

———, "Before Adam." New York, The Macmillan Company, 1923.

Lustig, "Ist die fuer Gifte erworbene Immunitaet uebertragbar

von Eltern auf die Nachkommenschaft?" *Zentralblatt fuer allgemeine Pathologie* XV, Nr. 6, 1904.

MAAS, OTTO, "Versuche ueber Umgewoehnung und Vererbung beim Seidenspinner." *Archiv fuer Entwicklungsmechanik* XLI, p. 672, 1915.

MACBRIDE, E. W., "The Study of Heredity," Part IV. *The Eugenics Review,* January, 1917 a.

———, "Recapitulation as a Proof of the Inheritance of Acquired Characters." *Scientia* XXII, pp. 425-434, December, 1917 b.

———, "The Method of Evolution." *Scientia* XXVIII, pp. 23-33, 1920.

———, "The Inheritance of Acquired Characters and Its Bearing on Eugenic Theory and Practice." *Eugenics Review*, July, 1922.

———, "Experiments on Ciona Intestinalis." *Nature* (London), No. 2821, pp. 759-760, November, 1923. See here also confirmation of Kammerer's Salamandra experiment.

———, *Introduction to the Study of Heredity.* London, 1924.

———, and A. JACKSON, "The Inheritance of Colour in the Stick-Insect, Carausius Morosus." *Proceedings of the Royal Society*, vol. 89, 1915.

MACCLENDON, J. F., "Protozoan Studies." *Journal of Experimental Zoology* VI, Nr. 2, pp. 265-284, 1909.

MACDOUGAL, D. T., "The Induction of New Species." *Science*, N. S. V., 23, 1906; 27, 1908; 33, 1911 a.

———, "Alterations in Heredity Induced by Ovarial Treatment." *Bot. Gazette* LI, 1911 b.

MACFERLANE, "A Comparison of the Minute Structure of Plant Hybrids with That of Their Parents, and Its Bearing on Biological Problems." *Transactions of the Royal Society of Edinburgh* XXXVII, pp. 203-286, 1895.

Maciesza, A., and A. Wrzosek, "Experimentelle Untersuchungen ueber die Vererbung der durch Ischiadicus-Verletzung hervorgerufenen Brown-Séquardschen Meerschweinchen-Epilepsie." *Archiv fuer Rassen- und Gesellschaftsbiologie* 1911a.

———, "Experimente und Beobachtungen, welche beweisen, dass die durch Verletzung des Nervus ischiadicus hervorgerufenen Verunstaltungen der hinteren Extremitaeten bei Meerschweinchen und weissen Maeusen nicht vererbt werden." *Archiv fuer Rassen- und Gesellschaftsbiologie* 1911 b.

Magnus, Wilhelm, "Transplantation of Ovaier med soerligt Hensyn til Afkommet." *Norsk magazin for laegevidenskaben*, Nr. 9, 1907.

Martins, "Das pathogenetische Vererbungsproblem." *Medizinische Klinik*, Berlin, VI, pp. 7-9, 1910.

Mendel, Gregor, "Versuche ueber Pflanzenhybriden." *Ostwald's Klassiker der exakten Wissenschaften*, Nr. 121, Leipzig, W. Engelmann, 1901. See also Bateson, W., *Mendel's Principles of Heredity*. Cambridge, University Press, 1909.

Meyer, A., and E. Schmidt, "Ueber die gegenseitige Beeinflussung der Symbionten heteroplastischer Transplantationen, mit besonderer Beruecksichtigung der Wanderung der Alkaloide durch die Pfropfstellen." *Flora* C, 317-395, 1910.

Milewski, "Tornier's experimentelle Untersuchungen ueber das Entstehen pathologischer Verbildungen bei Tieren sowie Weiteres ueber experimentelles Erzielen von monstroesen Goldfischarten." *Archiv fuer Entwicklungsmechanik* XLIV, Nr. 3-4, pp. 472-498, 1918.

Millot, J., "Le Pigment purique chez les Vertèbrés Inférieurs." *Bulletin Biologique de la France et la Belgique* LVII, p. 333, 1923.

Mingazzini, "Sulla rigenerazione nei Tunicati." *Boll. Soc.*

Nat. Napoli (1), Anno 5, pp. 76, 1891. See also *Neapler Jahresberichte* 1891, *Tunicata*, p. 5, and *Nature* (London), Nr. 2825, 22. Dec., 1923, p. 899. Letter by H. Przibram to W. Bateson.

MITCHELL, CLAUDE W., and POWERS, J. H., "Transmission Through the Resting Egg of Experimentally Induced Characters in Asplanchna Amphora." *Journal of Experimental Zoology* XVI, pp. 347-396, 1914.

MORGAN, LLOYD, *Habit and Instinct.* London, 1896.

MORGAN, T. H., "Growth and Regeneration in Planaria Lugubris." *Archiv fuer Entwicklungsmechanik* XIII, 178-212, 1902.

———, "Cross- and Self-Fertilization in Ciona Intestinalis." *Archiv fuer Entwicklungsmechanik* XXX (Festband fuer Roux), Part II, pp. 206-235, 1910.

———, *The Physical Basis of Heredity.* Philadelphia and London, J. B. Lippincott Co., 1919.

NOWIKOFF, M., "Ueber die Wirkung des Schilddruesenextraktes und einiger anderer Organstoffe auf Ciliaten." *Archiv fuer Protistenkunde* XI, p. 309, 1908.

NUSSBAUM, M., *Ueber Vererbung.* Bonn, Cohen, 1888.

OBERSTEINER, HEINRICH, "Zur Frage der hereditaeren Ubertragbarkeit acquirierter pathologischer Zustaende." *Neurologisches Zentralblatt*, Nr. 11, 1900.

ORSCHANSKY, J., *Die Vererbung im gesunden und krankhaften Zustande.* Stuttgart, Ferd. Enke, 1903.

OSBORN, H. F., "The Palaeontological Evidence for the Transmission of Acquired Characters." *Nature*, 1889; *American Naturalist*, 1889 and 1891.

OSTWALD, WILHELM, *Grosse Naenner. Studien zur Biologie des Genies.* Leipzig, Akademische Verlagsgesellschaft.

OSTWALD, WOLFGANG, "Experimentelle Untersuchungen ueber den Saisondimorphismus bei Daphniden." *Archiv fuer*

Entwicklungsmechanik XVIII, Nr. 3, pp. 415-451, 1904.

PAPANICOLAU, G., "Ueber die Bedingungen der sexuellen Differenzierung bei Daphniden" and "Experimentelle Untersuchungen über die Fortpflanzungsverhältnisse der Daphniden (Simocephalus vetulus und Moina rectirostris var. Lilljeborgii)." *Biologisches Zentralblatt* XXX, 1910.

PAULY, A., *Darwinismus und Lamarckismus.* Muenchen, E. Reinhardt, 1905.

PAVLOV, I. P., "New Researches on Conditioned Reflexes." *The Bulletin of the Battle Creek Sanitarium and Hospital Clinic* XIX, Nr. 1, pp. 1-4, December, 1923.

PAYNE, F., "Forty-nine Generations in the Dark." *Biological Bulletin* XVIII, Nr. 4, 1910.

———, "Drosophila Ampelophila Bred in the Dark for Sixty-nine Generations." *Biological Bulletin* XXI, 1911.

PEARL, RAYMOND, "Biology of Death." Philadelphia, J. B. Lippincott Co., 1922.

PERKINS, M., *Nature* (London), vol. 112, Nr. 2807, August 18, 1923.

PETERS, W., "Ueber Vererbung psychischer Faehigkeiten." *Fortschritte der Psychologie* III, Nr. 4-6, Leipzig-Berlin, B. G. Teubner.

PFEFFER, W., *Die periodischen Bewegungen der Blattorgane,* Leipzig, 1875.

———, "Die Entstehung der Schlafbewegungen bei Pflanzen." *Biologisches Zentralblatt* XXVIII, 1908.

———, "Untersuchungen ueber die Entstehung der Schlafbewegungen der Blattorgane." *Abhandlungen der mathematisch-physikalischen Klasse der saechsischen Gesellschaft der Wissenschaften* XXX, 1907; XXXII, 1911.

PICTET, ARNOLD, "Influence de l'alimentation et de l'humidite sur la variation des papillons," *Memories de la Société*

de Physique et d'Histoire Naturelle de Genève XXXV, 1905.

———, "Quelques exemples de l'herédité des charactères acquis." *Verhandlungen der Schweizer Naturforschenden Gesellschaft* I, 1910.

———, "Un nouveau exemple de l'herédité des charactères acquis." *Arch. scienc. phys. et nat.* XXXI, 1911.

———, "Action du milieu et herédité." *Comptes rendus de la Société Génève* XXXVIII, pp. 64-67, 1921.

PLATE, LUDWIG, "Variabilitaet und Artbildung nach dem Prinzip geographischer Formenketten bei den Cerion-Landschnecken der Bahama-Inseln." *Archiv fuer Rassen- und Gesellschaftsbiologie* IV, 1907.

———, *Selektionsprinzip und Probleme der Artbildung.* Leipzig, W. Engelmann, 4th Edition, 1913.

PLOETZ, ALFRED, *Die Tuechtigkeit unserer Rasse.* Berlin, Gustav Fischer, 1895.

POGONOWSKA, IRENE, "Ueber den Einfluss chemischer Faktoren auf die Farbveraenderungen des Feuersalamanders." *Archiv fuer Entwicklungsmechanik* XXXIX, p. 352, 1914.

POWERS, J. H., "The Causes of Acceleration and Retardation in the Metamorphosis of Amblystoma Tigrinum." *American Naturalist* XXXVII, pp. 385-410, 1903.

PRINGSHEIM, H., *Die Variabilitaet niederer Organismen.* Berlin, J. Springer, 1910.

PRZIBRAM, HANS, "Experimentelle Studien ueber Regeneration II. Antedon." *Archiv fuer Entwicklungsmechanik* II, p. 321, 1901.

———, "Aufzucht, Farbwechsel und Regeneration der Gottesanbeterinnen (Mantidae) III." *Archiv fuer Entwicklungsmechanik* XXVIII, 1909.

———, "Experimentalzoologie." *III. Band Phylogenese inklusive Hereditaet.* Leipzig-Wien, Verlag F. Deuticke, 1910 a.

———, "Versuche an Hitzeratten." *Verhandlungen der Gesellschaft deutscher Naturforscher und Aerzte*, Salzburg, II., 1. Haelfte, pp. 178, 180, 1910 b. (See also *Akademischer Anzeiger, Vienna,* 1923.)

———, "Die Umwelt des Keimplasmas I: Das Arbeitsprogramm." *Archiv fuer Entwicklungsmechanik* XXXIII, 1912.

———, "Die Umwelt des Keimplasmas VI, Direkte Temperaturabhaengigkeit der Koerperwaerme bei Ratten." *Archiv fuer Entwicklungsmechanik* XLIII, pp. 37-46, 1917.

———, "Die Methode autophorer Transplantation." *Archiv fuer Mikroskopische Anatomie und Entwicklungsmechanik* XCIX, Nr. 1, pp. 1-14, 1923 a.

———, *Temperatur und Temperatoren im Tierreiche.* Wien-Leipzig, F. Deuticke, 1923 b.

Przibram, Hans, and Brecher, Leonore, "Die Farbmodifikationen der Stabheuschrecke Dixippus morosus." *Archiv fuer Entwicklungsmechanik* L, Nr. 1-2, 1922.

———, and Dembowski, Jan, "Der Einfluss gelber und schwarzer Umgebung der Larve auf die Fleckenzeichnung des Vollmolches von Salamandra maculosa Laur., forma typica." *Archiv fuer Entwicklungsmechanik* L, Nr. 1-2, 1922.

Redfield, Casper L., *Control of Heredity.* Chicago-Philadelphia, Monarch Book Company, 1903.

———, *Dynamic Evolution.* New York-London, G. P. Putnam's Sons, 1914.

———, *Great Men and How They Are Produced.* Chicago, 1915.

———, "Selection Not a Factor of Evolution." *The Eclectic Medical Journal*, November, 1919.

———, "The Germ-Plasm." *Medical Record*, September 4, 1920.

———, *Human Heredity.* Chicago, Heredity Publishing Company, 1921.

———, "The Human Power Plant." *The Medical Times,* July, 1923 a.

———, "The Inheritance of Acquired Characteristics. Biologoists under Cross Examination." *The Horse Review,* p. 1222, December 12, 1923 b.

———, "Acquired Immunity." *American Medicine* (New Series) XVIII, Nr. 9, pp. 661-665, September, 1923. See also *American Botanist,* pp. 94-100, 1923 c.

RIDDLE, OSCAR, "Studies with Sudan III in Metabolism and Inheritance." *Journal of Experimental Zoology* VIII, pp. 163-184, 1910.

RIGNANO, EUGENIO, *Upon the Inheritance of Acquired Characters. A Hypothesis of Heredity, Development and Assimilation.* Authorized English Translation, by B. C. H. Harvey, Chicago, The Open Court Publications, 1911.

RITZEMA BOS, T., "Zur Frage der Vererbung von Traumatismen." *Biologisches Zentralblatt* XI, 1891.

ROMANES, GEORGE, *Mental Evolution in Man.* London, 1888.

———, *Darwin and After.* In the second volume Corroboration of the Experiments of Brown-Séquard. London, 1892-1897.

ROMEIS, B., "Der Einfluss verschiedenartiger Ernaehrung auf die Regeneration der Kaulquappen." *Archiv fuer Entwicklungsmechanik* XXXVII, p. 183, 1913; XL, p. 571, 1914; XLI, pp. 57-119, 1915.

———, "Experimentelle Untersuchungen über die Wirkung innersekretorischer Organe." *Zeitschrift für die gesamte experimentelle Medizin* VI, pp. 101-288, 1918.

———, "Geschlechtszellen oder Zwischenzellen?" *Klinische Wochenschrift* I, Nr. pp. 19-21, 1922.

ROUX, WILHELM, "Ueber die bei der Vererbung von Variationen anzunehmenden Vorgaenge." *Vortraege und*

Aufsaetze ueber Entwichlungsmechanik XIX, 2d Edition, Leipzig-Berlin, W. Engelmann, 1913.

RUTTMAN, W. J., *Erblichkeitslehre und Paedagogik.* Leipzig, A. Haase, 1917.

RUZICKA, V., *Restitution und Vererbung.* Berlin, J. Springer, 1919. Furthermore a Handbook of Genetics, written in Czechik language, Prag. Alois Hynek, 1914.

———, "Hysteresis des Protoplasmas und das Problem der Verjuengung." *Deutsche medizinische Wochenschrift* XLVIII, p. 931, July, 1922.

SARASIN, P., and F., *Die Landmollusken von Celebes.* Wiesbaden, 1899.

SCHALLMAYER, W., *Vererbung und Auslese in ihrer soziologischen und politischen Bedeutung.* Jena, G. Fischer, 2d Edition, 1910.

SCHAXEL, J., *Archiv fuer soziale Hygiene* VIII, p. 137, 1913.

———, "Ueber den gegenwaertigen Stand der Frage nach der Vererbung erworbener Eigenschaften." *Deutsche medizinische Wochenschrift* XLVII, p. 535, 1921.

SCHILLER, J., "Vorversuche zu der Frage nach der Vererbung erworbener Eigenschaften." *Archiv fuer Entwicklungsmechanik* XXXIV, 1912.

———, "Ueber somatische Induktion auf die Keindrüsen bei den Säugetieren I." *Archiv fuer Entwicklungsmechanik* XXXVIII, pp. 136-143, 1914.

SCHLEIP, P., "Ueber den Einfluss des Lichtes auf die Faerbung von Dixippus und die Frage der Erblichkeit des erworbenen Farbkleides." *Zoologischer Anzeiger* LII, pp. 151-160, 1921.

SCHNEIDER, K. C., *Einfuehrung in die Deszendenztheorie.* Jena, G. Fischer, 2d Edition, 1911.

SCHROEDER, CHRISTIAN, "Die Zeichnungsvariabilitaet von Abraxas grossulariata." *Allgemeine Zeitschrift fuer Entomologie* VIII, 1903 a.

———, *Ueber experimentell erzeugte Instinktvariationen.* Verhandlungen der Zoologischen Gesellschaft, Leipzig, 1903 b.

Schroeder, P., "Versuche mit chronischer Alkoholintoxikation bei Kaninchen." *M.-S. Psych. Neurol.* XXXVI, Nr. 1, pp. 1-12, 1913.

Schuckmann, Waldemar von, "Ueber die Einwirkung niedriger Temperaturen auf den Fortgang der inneren Metamorphose bei der Puppe von Vanessa urticae." *Archiv fuer Entwicklungsmechanik* XXVII, pp. 513-559, 1909.

Schuebeler, F. C., *Viridarium Norwegicum.* Christiania, 1885. (Report in the *Biologisches Zentralblatt*, 1886.)

Schultz, Eugen, "Ueber Reduktionen." "III: Die Reduktion und Regeneration des abgeschnittenen Kiemenkorbes von Clavellina lepadiformis." *Archiv fuer Entwicklungsmechanik* XXIV, pp. 503-523, 1907.

Schweizerbarth, Melitta von, *Eine rote Farbenvarietaet von Salamandra maculosa Laur.* Bericht der Senckenbergischen Naturforschenden Gesellschaft, Frankfort a.M., pp. 119-121, 1906.

Secerov, Slavko, "Die Umwelt des Keimplasmas II. Der Lichtgenuss im Salamandra-Koerper." *Archiv fuer Entwicklungsmechanik* XXXIII, 1912. "III. Der Lichtgenuss im Lacerta-Köeper." *Archiv fuer Entwicklungsmechanik* XXXIV, p. 742, 1912.

———, "Ueber das Farbkleid von Feuersalamandern, deren Larven auf gelbem oder schwarzem Untergrunde gezogen waren." *Biologisches Zentralblatt* XXXIV, pp. 339-344, 1914.

Semon, Richard, "Beweise fuer die Vererbung erworbener Eigenschaften." *Archiv fuer Rassen- und Gesellschaftsbiologie* IV, 1907.

———, "Ueber die Erblichkeit der Tagesperiode." *Biologisches Zentralblatt* XXV, 1905; and "Hat der Rhyth-

mus der Tageszeiten bei Pflanzen erbliche Eindruecke hinterlassen?" *Biologisches Zentralblatt* XXVIII, 1908.

———, *The Mneme.* London, Allen and Unwin, 1921; and *Die mnemischen Empfindungen.* Leipzig, W. Engelmann, 1909.

———, "Die Stand der Frage nach der Vererbung erworbener Eigenschaften." *Abderhaldens Fortschritte der naturwissenschaftlichen Forschung* II, 1911.

———, *Das Problem der Vererbung erworbener Eigenschaften.* Leipzig, Verlag, W. Engelmann, 1912.

———, "Die Fuszsohle des Menschen. Eine Studie ueber die unmittelbare und ueber die erbliche Wirkung der Funktion." *Archiv fuer mikroskopische Anatomie*, 2 Abt., LXXXII, pp. 164-211, 1913.

———, "Karl Toldt's Untersuchungen vorgeschrittener Entwicklungsstadien von Saeugetierembryonen." *Die Naturwissenschaften,* pp. 287-288, 1916.

———, "Ueber das Schlagwort 'Lamarckismus'." *Zeitschrift fuer induktive Abstammungs- und Vererbungslehre* XII, pp. 51-52, 1919–1920.

Shattock, S. G., "Lamarckism and Callosities." *Proceedings Royal Society of Medicine* IV, 1911.

Sitowski, L., "Contribution à la biologie des teignes." *Bulletin Internationale de l'Académie des Sciences*, Cracovie, 1905.

———, "On the Inheritance of Aniline Dye." *Science*, N.S., XXX, p. 308, Sept. 3, 1909.

Sommer, M., "Die Brown-Séquardsche Meerschweinschenepilepsie und ihre erbliche Uebertragung auf die Nachkommen." *Ziegler's Beitraege zur Pathologischen Anatomie und allgemeinen Pathologie* XXVII, 1900.

Spemann, H., "Die Erzeugung tierischer Chimaeren durch heteroplastische embryonale Transplantation zwischen Triton cristatus und taeniatus." *Archiv fuer Entwicklungsmechanik* XLVIII, pp. 533-570, 1921.

STANDFUSS, M., "Experimentelle zoologische Studien mit Lepidopteren." *Neue Denkschriften der allgemeinen Schweizerischen Gesellschaft fuer die gesamten Naturwissenschaften* XXXVI, 1898.

———, "Gesamtbild der bis Ende 1898 an Lepidopteren vorgenommenen Temperaturversuche." *Insektenboerse* XI, 1899.

STEINACH, E., "Feminierung von Maennchen und Maskulierung von Weibchen." *Zentralblatt fuer Physiologie* XXVII, Nr. 14, 1913.

———, *Verjuengung durch experimentelle Neubelebung der alternden Pubertaetsdruese.* Berlin, Verlag J. Springer, 1920.

STEINACH, E., and KAMMERER, P., "Klima und Mannbarkeit." *Archiv fuer Entwicklungsmechanik* XLVI, Nr. 2-3, 1920.

STIEVE, HANS, "Ueber den Einfluss der Umwelt auf die Eierstoecke der Tritonen: ein Beitrag zur Frage nach der Vererbbarkeit erworbener Eigenschaften und der Parallelinduktion." *Archiv fuer Entwicklungsmechanik* XLIX, pp. 79-268, 1921.

STINGL, GEORG, "Experimentelle Studie ueber die Ernaehrung von pflanzlichen Embryonen." *Flora oder allgemeine botanische Zeitung*, vol. 97, Nr. 3, 1907.

STOCKARD, CHARLES B., "The Development of Artificially Produced Cyclopean Fish: The Magnesium Embryo." *Journal of Experimental Zoology* VI, pp. 285-340, 1909.

STOCKARD, CHARLES R., and CRAIG, DOROTHY M., "An Experimental Study of the Influence of Alcohol on the Germ Cells and the Developing Embryos of Mammals." *Archiv fuer Entwicklungsmechanik* XXXV, Nr. 3, pp. 569-584, 1912. See also *Archives of Internal Medicine* X, pp. 369-398, 1912, and *American Naturalist* XLVII, pp. 641-682, 1913.

STOCKARD, CHARLES R., and PAPANICOLAU, G. N., "Further Studies on the Modification of the Germ Cells in Mammals: The Effect of Alcohol on Treated Guinea Pigs and Their Descendants." *Journal of Experimental Zoology* XXVI, pp. 119-276, 1918.

STRASSER, H., *Fragen der Entwicklungsmechanik: die Vererbung erworbener Eigenschaften.* Bern and Leipzig, E. Bircher, 1920.

SUMNER, F. B., "The Reappearance in the Offspring of Artificially Produced Parental Modifications." *American Naturalist* XLIV, 1910 a.

———, "An Experimental Study of Somatic Modifications and Their Reappearance in the Offspring." *Archiv fuer Entwicklungsmechanik* XXX (Festband fuer W. Roux), Part II, 1910 b; *Journal of Experimental Zoology* XV, pp. 315-378, 1913; XVIII, pp. 325-433, 1915.

SWINGLE, W. W., "The Acceleration of Metamorphosis in Frog Larvæ by Thyroid Feeding." *Journal of Experimental Zoology* XXIV, pp. 521-544, 1918; XXXIV, 119-142, 1921.

TAUBE, ERWIN, "Regeneration mit Beteiligung ortsfremder Haut bei Tritonen." *Archiv fuer Entwicklungsmechanik* XLIX, pp. 269-315, 1921.

THOMSON, J. A., *Heredity.* New York, G. P. Putnam's Sons; London, John Murray, 1908.

TIRALA, L. G., "Regeneration und Transplantation bei Criodrilus." *Biologisches Zentralblatt* XXXII, Nr. 1, 1912, and *Archiv fuer Entwicklungsmechanik* XXXV, pp. 523-554, 1913.

TIZZONI and CATTANO, "Ueber die erbliche Uebertragung der Immunitaet gegen Tetanus." *Deutsche medizinische Wochenschrift,* Nr. 18, 1892.

TORNIER, GUSTAV, "Ueber Hyperdaktylie, Regeneration und

Vererbung mit Experimenten." *Archiv fuer Entwicklungsmechanik* III, pp. 469-476, 1896.

———, "Vorlaeufiges ueber das Entstehen der Goldfischrassen." *Sitzungsberichte der Gesellschaft Naturforschender Freunde*, Berlin, p. 40, 1908.

TOWER, W. L., *An Investigation of Evolution in Chrysomelid Beetles of the Genus Leptinotarsa.* Carnegie Institution Publication, Nr. 48, Washington, 1906. See also *Biological Bulletin*, Woods Hole, XVIII, pp. 285-352, 1910.

TSCHERMAK, ARMIN VON, "Ueber den Einfluss der Bastardierung auf Form, Farbe und Zeichnung von Kanarieneiern." *Biologisches Zentralblatt* XXX, pp. 641-648, 1910.

———, "Ueber die Wirkung von Bastardierung auf die Vogeleischale: Xeniodochie und Telegonie." *Prager medizinische Wochenschrift* XL, pp. 257-259, 1915 a.

———, "Ueber Verfaerbung von Huehnereiern durch Bastardierung und die Nachdauer dieser Farbveraenderung (Farbxenien und Faerbungstelegonie)." *Biologisches Zentralblatt* XXXV, pp. 46-63, 1915 b.

———, "Ueber das verschiedene Ergebnis reziproker Kreuzung von Huehnerrassen und ueber dessen Bedeutung fuer die Vererbungslehre (Theorie der Anlagenschwaechung oder Genasthesie)." *Biologisches Zentralblatt* XXXVII, pp. 217-277, 1917.

UHLENHUTH, EDUARD, "Die synchrone Metamorphose transplantierter Salamanderaugen." *Archiv fuer Entwicklungsmechanik* XXXV, p. 211, 1913.

———, "Der Einfluss des Wirtes auf das transplantierte Amphibienauge." *Archiv fuer vergleichende Ophthalmologie* III, 1913. See also *Congress of Anatomists*, St. Louis, Dec., 1914.

———, "A Further Contribution to the Metamorphosis of

Amphibian Organs: The Metamorphosis of Grafted Skin and Eyes of Amblystoma Punctatum." *Journal of Experimental Zoology* XXIV, pp. 237-302, 1917.

——, "Further Facts Regarding the Influence of Feeding the Anterior Lobe of Hypophysis on the Rate of Growth and the Size of Amblystoma Tigrinum." *Journal of Experimental Zoology* XXXVII, Nr. 1, pp. 101-114, 4 figg., January, 1923.

Unruh, C. M. von, "Ein merkwuerdiger Fall von Vererbung." *Kosmos, Handweiser fuer Naturfreunde*, VIII, p. 144, 1911.

Vries, H. de, *Die Mutationstheorie*, Leipzig, 1901 and 1903; *Arten und Varietaeten*, Berlin, 1906; *Die Mutationen in der Erblichkeitslehre*, Berlin, Borntraeger, 1912.

Walker, Quotation according to Friedrich Hertz.

Weigl, R., "Ueber homoeoplastische und heteroplastische Hauttransplantation bei Amphibien mit besonderer Beruecksichtigung der Metamorphose." *Archiv fuer Entwicklungsmechanik* XXXVI, 1913.

Weismann, August, *Aufsaetze ueber Vererbung und verwandte biologische Fragen*. Jena, G. Fischer, 1892.

——, *The Germ Plasm. A Theory of Heredity*. Translated by W. Newton Parker. London, The Walter Scott Publishing Company, and New York, Charles Scribner's Sons, 1915.

——, *The Evolution Theory*. Translated by T. A. Thomson and Margaret R. Thomson. London, Edw. Arnold, two volumes, 1904.

Werner, Franz, Report about Curt Herbst. *Zoologisches Zentralblatt*, 1923.

Westphal, C., "Ueber kuenstliche Erzeugung von Epilepsie bei Meerschweinchen." *Berliner klinische Wochenschrift* VIII, 1871.

WETTSTEIN, R. VON, "Ueber direkte Anpassung." Lecture in the Academy of Sciences, Vienna, May 29, 1902.

———, *Der Neo-Lamarckismus und seine Beziehungen zum Darwinismus.* Jena, 1903.

WIEDEMANN, MAX, "Ueber Faerbungsveraenderungen bei Salamandra maculosa unter dem Einfluss dunkler Bodenfarbe und Feuchtigkeit." *Zoologischer Anzeiger* XXXVII, pp. 179-180, 1911.

WIEDERSHEIM, ROBERT, *Vergleichende Anatomie der Wirbeltiere*, 7th Edition. Jena, G. Fischer, 1909.

WIESNER, BERTHOLD P., "Die Funktionsfaehigkeit autophor transplantierter Ovarien bei Ratten." *Archiv fuer mikroskopische Anatomie und Entwicklungsmechanik*, vol. 99, Nr. 1, pp. 140-149, 1923.

WIGGAM, ALBERT EDWARD, *The New Decalogue of Science.* Indianapolis, The Bobbs-Merrill Company, publishers, 1923.

WILCKENS, M., "Die Vererbung erworbener Eigenschaften vom Standpunkt der landwirtschaftlichen Tierzucht inbezug auf Weismann's Theorie der Vererbung." *Biologisches Zentralblatt* XIII, pp. 420-427, 1893.

WILHELM, OTMAR, "Contribucion al estudio histiofisiologico de los llamados fenomenos de Rejuvenecimento." *Segunda Nota Rivista medica di Chile* LI, Nrs. 7, 8, 1923.

WILLE, N., "Ueber die Schuebelerschen Anschauungen in betreff der Veraenderungen der Pflanzen in noerdlichen Breiten." *Biologisches Zentralblatt* XXV, 1905.

WINKLER, H., "Ueber die Nachkommenschaft der Solanum-Pfropfbastarde und die Chromosomenzahl ihrer Keimzellen." *Zeitschrift fuer Botanik* II, pp. 1-23, 1909; See also *Berichte der Deutschen Botanischen Gesellschaft* XXVIII, pp. 116-118, 1910.

———, *Untersuchungen ueber Pfropfbastarde.* Jena, G. Fischer, beginning from 1912.

WOLF, F., "Ueber Modifikationen und experimentell ausgeloeste Mutationen von Bacillus prodigiosus und anderen Schizomyzeten." *Zeitschrift fuer induktive Abstammungs- und Vererbungslehre* II, 1909.

WOLK, VAN DER, Quotation according to J. H. Kellogg.

WOLTERRECK, R., "Ueber Veraenderung der Sexualitaet bei Daphniden." *Internationale Revue der gesamten Hydrobiologie und Hydrographie* IV, 1911 a.

———, "Beitrag zur Analyse der Vererbung erworbener Eigenschaften: Transmutation und Praeinduktion bei Daphnia." *Verhandlungen der Deutschen Zoologischen Gesellschaft* XX, Leipzig, 1911 b.

ZEDERBAUER, E., "Versuche ueber Vererbung erworbener Eigenschaften bei Capsella bursa pastoris." *Oesterreichische Botanische Zeitschrift* LVIII, 1908.

———, "Alter und Vererbung." *Zeitschrift fuer Pflanzenzuechtung* V, pp. 257-259; *Verhandlungen der Zoologisch-botanischen Gesellschaft Wien*, pp. 81-87, 1917.

ZIEGLER, E., "Koennen erworbene pathologische Eigenschaften vererbt werden, und wie entstehen erbliche Krankheiten und Missbildungen?" *Ziegler's Beitraege zur patholoigischen Anatomie und Physiologie* I, 1886; IV, 1889.

ZIEGLER, H. E., "Die Streitfrage der Vererbungslehre (Lamarckismus oder Weismannismus?)." *Naturwissenschaftliche Wochenschrift*, N.F. IX, Nr. 13, 1910.

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